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# Back to the Beginning: An Empiricist Defense of Scientific Stories About the Past

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A thesis submitted in partial fulfillment of the requirements for the Doctor of Philosophy degree in Philosophy

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## Abstract

The Earth has not always been accompanied by its celestial partner, the Moon. In fact, the Moon was acquired by the Earth about 100 million years after the start of the solar system. It was acquired in the aftermath of a massive collision between the Earth and another planet, dubbed “Theia,” the mythological mother of the Greek goddess of the Moon. Most of the iron-rich cores of Earth and Theia merged almost immediately, while the rest of the two planets vaporized. Some of the impact ejecta was lost, but enough remained in gravitationally bound orbit around what was then 98% of current-sized Earth. What remained of the impact ejecta derived from the outer, silicate portion of the Earth, cooled rapidly, condensed and accreted to form the body we are so accustomed to seeing in our sky.

I want to know how we can know all of *that*. Hempel (1942) famously argued that theories and hypotheses concerning history cannot meet the standard of a true scientific theory, and can, at best, only offer explanation sketches, i.e. how-possible scenarios, and at worst, amount to nothing more than in-principle untestable tales, i.e. just-so stories. This seed of distrust concerning scientific theories that have a narrative form has persisted, even if no one still thinks that Hempel’s explication of what makes a theory justly scientific is adequate.

Yet we do seem to have much genuine scientific knowledge of the deep past. Such knowledge must, then, be justified or confirmed on the basis of some alternative inferential methodology that both makes up for the inability of historical scientists to perform controlled laboratory tests on past complex events and which makes the narrative form of theories inessential. This dissertation develops a new empiricist framework that answers Hempel’s challenge to the untestability of stories concerning the deep past.

**Keywords:** Historical Science, Scientific Stories, Inferential Methodology, Testing, Reconstructing the Deep Past, Geosciences, Cosmology

## Summary for Lay Audience

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## **Dedication**

This one's for the birds.

To my Jay and Phoebe: you kept my spirits aloft in the dark parts of this long journey.

And to Deborah: you defended our nest and held us together through many storms.

I could not have done this without you.

## Acknowledgements

I have benefited greatly from the guidance of my supervisor, Chris Smeenk. His feedback has been invaluable and constructive. At so many junctures, I had lost my grip on the philosophical thread and Chris was always there to help me recover it. To Chris, without your gentle prodding, this would have been nothing more than a strange kind of review article, written by an outsider. But you continually pressed me to come back to and sharpen the philosophical point. As a member of his research group I have also had the opportunity to talk with both philosophers and scientists from a wide range of relevant areas that I would not have had otherwise. I hope the present work reflects the richness of the perspective I now have that is a result of so many positive interactions. In this I also have the Templeton Foundation to thank for their financial support through the "New Directions in Philosophy of Cosmology" grant. As well, the present work has been financially supported by the Rotman Institute of Philosophy, which I have called home throughout.

I wish also to express my gratitude to my mentor, George Smith. The present work began life as a seminar paper for the seminar he led—along with Patrick Forber (who I also thank)—during my MA at Tufts University entitled "Accessing Prehistory." Over the years since, George has invested a tremendous amount in me. Without George's early support and intervention, I would not have been successful as it was not clear to others that this was a genuine philosophical project, let alone an interesting one. At many points of despair, George had just the right word of encouragement to keep me going. In addition to the moral support, George's background and willingness to discuss at length various engineering examples and accident reconstruction cases were instrumental in shaping my view. A more gracious, supportive and knowledgeable mentor cannot be found. To George, you have taught me very much and through it all you helped me find my voice, and you supported me as I sought a new path through uncharted waters.

I would also like to thank Dillen Moonen for his important help. Dillen was my neighbor during the crucial period when Chapter's 3 & 4 were coming together. This was a difficult

stretch for us, financially, and he generously donated his time and expertise to keeping our now 20 year-old Volkswagen in running order. More than this, though, Dillen has a gifted mind in terms of diagnosis of dynamical systems and he allowed me to rather relentlessly prod and question. I gained many insights into the nature of traces and diagnostic features that play significant roles in the present work. Indeed, I could not have seen the makings of the argument concerning inflationary cosmology in Chapter 4 without the many hours Dillen spent talking and teaching me about automotive issues as we poured over my old car. Finally, during the height of the pandemic when my time was running out to finish and I was languishing, on lockdown, with our family of four confined to our very small apartment, Dillen invited us to stay at his father's farmhouse in Blue Mountain, ON. This was such a joy and it was there that I was able to find the thread that makes Chapter 3 coherent and the argument of Chapter 4 was finally hatched. I thank also Peter Moonen and Dorothea Smet for allowing us to stay in their home while they were stuck abroad during the pandemic. Thank you all for your generosity.

I would also like to thank Marie Gueguen for her support and encouragement. A better friend cannot be had. You showed me the way.

lastly, I would like to thank my family. As I said in the Dedication, I could not have done this without you. Without your love and support I would have lost my way forever in the darkness that is writing a doctoral dissertation. Many personal dragons had to be slain, the biggest being my lack of confidence. Your love and encouragement made this possible. To Jay and Phoebe, may you find strength in the future in having walked this path with me. We did this together. To Deborah, you made this journey possible with your love and hard work, carrying so many great burdens that afforded me the time I needed to complete this. Thank you. Finally, I wish to thank my mom and dad. Their support, especially early on when I decided to pursue graduate studies and needed their help caring for our newborn son was essential. Alas, my dad did not live to see the end, but his voice is within me and so his mark on this work is immense, as is my mother's.

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# Chapter 1

## Rethinking the distinction between historical and experimental science

### 1.1 Introduction

A great deal of the science currently done in science departments appears, at least *prima facie*, different from the kinds of scientific investigation that have been the focus of most philosophers of science. Sciences that investigate the past have lately been lumped together under the category “historical sciences” (see, e.g. Cleland (2002, 2011, 2001); Jeffares (2008); Currie (2018); Tucker (2004); Kosso (2001); Turner (2007, 2005); Inkpen (2008); Chapman & Wylie (2016); Bromham (2016); O’Malley (2016)). This term is supposed to distinguish sciences, such as geology and paleontology, from more traditional kinds of science, such as physics and chemistry, or so-called “experimental sciences.” The idea behind this classification is that investigating the remote past constitutes a different kind of inquiry than, say, investigating the constitution of matter or the genetic basis of life. Though the moniker “historical science” is relatively new, the thought that investigating the past constitutes a different kind of scientific investigation is not. (See, e.g. Hempel (1942); Toulmin (1962) and (Gould, 2002, Ch.3), who argued that one of Charles Darwin’s profound accomplishments was the articulation of a

historical method, which I discuss further below.)

In both the scientific and philosophic literature there are four views or motivations to confront the *prima facie* distinctiveness of sciences whose subject matter focuses on the reconstruction of past events and states of affairs. The first view, most common amongst the more laboratory-minded scientists and philosophers, is that there is a difference in the kind and quality of evidence that these sciences can garner. Though the view is not often put into print, the feature of these sciences that is often seized upon as proof of their inferiority is that historical sciences, seemingly as a rule, produce narrative explanations, as opposed to characterizations of law-like relations, say. When reconstructing a history, one is reconstructing a *particular* history—often a sequence of events that had never happened before and that will never happen again—as opposed to searching for or describing universal phenomena. These sciences, therefore, cannot conduct experiments, for the phenomena under investigation happened so long ago, and the processes involved were typically so slow, operating over scales of energy, time and space that are simply too vast for humans to replicate them. It was in light of these observations concerning history that Hempel (1942) claimed that narrative histories can never achieve a status of more than an explanation sketch, for they can never conform to his standard of nomological explanation. The unpredictability of history, it seems, is the realm of contingency and uniqueness, not of regularity and experimentation. Moreover, because the subject of investigation is temporally located in the past, that investigators can do is to collect traces from long past happenings. Again, this is seldom put in print, but historical scientists are sometimes considered mere stamp-collectors, who have collected astounding and odd bits left over from the past, but that these collections don't really amount to genuine scientific knowledge. Whereas true science, on this view, aims at characterizing regularities in nature, historical science is the study of particulars. For those who hold this view, when scientists hypothesize about history they engage in a kind of “make believe” and so investigation of deep time needs to be done in an ahistorical way (see, e.g. Gee (2000); Goenner (2010)). In other words, either historical scientists are written off wholesale, or they are written off until a satisfactory

ahistorical methodology is developed that can empirically ground their claims.

The second view is a response to the first. Like the first, it agrees with the claim that historical sciences are epistemically inferior insofar as they ought to conform to the methodology of experiment. But, they argue, historical scientists simply pursue an alternative historical methodology. The game then is to articulate this methodology and to find arguments that justify calling the knowledge achieved concerning the deep past legitimately scientific. All of those cited two paragraphs ago (save for Hempel and perhaps Turner) share this presumption, viz. that historical scientific investigations employ an alternative methodology, and must do so to safeguard their status as *qua* sciences. Indeed, the view is that this alternative methodology places historical sciences on an epistemic par with experimental sciences. Philosophers are not alone in this approach, either. As mentioned, (Gould, 2002, Ch.3) and Gould (1986) makes such a case for paleobiology. Moreover Baker (1999) does so for geology, Unger & Smolin (2014) for cosmology, and Ćirković & Perović (2018); Anderl (2016, (2018) for astrophysics.

Those in the first first camp and some in the second tend to share a skeptical attitude toward narratives, seeing narrative explanation as a byproduct of the fact that historical accounts are diachronic and their appearance signals when the causal processes are too complex. On this view, the narrative is not an essential part of the scientific and evidential reasoning and the more “narrative” a reconstruction is, the more we should be suspicious of it. Such a view has been defended by (Kosso, 2001, 24), who declares, “Narrative explanation suits the situation of historiography, but not science.” Carol Cleland articulates this kind of sentiment more thoroughly:

Narrative explanation dominates thought about explanation in human history, where intangible human desires and purposes play key explanatory roles. It is also common in evolutionary biology and historical geology. The basic idea behind narrative explanation is to construct a story a coherent, intuitively continuous, causal sequence of events centering on a precipitating event and culminating in the phenomena (traces) in need of explanation. In some cases, the purpose is only to

establish the plausibility that certain sorts of causal processes could have given rise to the phenomena concerned; at best it represents a potential explanation. In other cases, however, the narrative is interpreted as showing how the phenomena actually came about. Because much is unknown about the events in the sequence narrative explanations have a significant fictional component, involving omissions and additions. This poses a potential problem insofar as it conflicts with the traditional emphasis in natural science on evidential warrant. The problem is exacerbated by the central role of explanation in the confirmation and disconfirmation of historical hypotheses. If the primary reason for accepting a historical hypothesis is its explanatory power and it draws its explanatory power primarily from the coherence and continuity of a quasi-fictional story, then historical natural science really does seem inferior to experimental science; in the absence of empirical warrant a narrative explanation amounts to little more than a just-so story. (Cleland, 2011)

Before moving on, I wish to clear up an ambiguity concerning the term “method.” In general, throughout this work, I mean the term in the sense of overarching inferential method, as in the so-called scientific method, which is an account (that I disagree with) of how hypotheses are tested. Another common use of the term is to pick out particular observational or experimental techniques. This is not the sense in which I use the term. My placement of Adrian Currie into this second approach might seem odd, given that he argues for a position he calls “methodological omnivory,” which is a highly promiscuous form of pluralism (Currie (2015, 2018)). I place him in this approach because, on the one hand, I read him as holding that this form of pluralism is, itself, distinctive of historical sciences. And on the other, as I’ll show below, he employs the machinery of the putative distinctive methodology to characterize the model of evidence he thinks is crucial, even if it does not exhaust all of the forms of evidence historical scientists reason from. Currie’s claim “there is no main business of historical science,” ((Currie, 2018, Ch.6)) indicates that he is not pursuing a normative account of the many different methods (in the second sense of “method” mentioned above) employed by historical

scientists. But Currie does aim to justify optimism for historical science by showing that investigations of the past are distinctive and take advantage of the fact that the world is such that there is pervasive evidential redundancy concerning the past, as I'll discuss below.

The third philosophical approach to sciences that investigate the past focuses on the narrative structure of theories of the past. Whereas the first two approaches tend to downplay the importance of narrative, this approach seeks to better understand both the narrative structure of reconstructions of the past and the role that the narrative plays. To be sure, not all philosophers who think that historical sciences utilize a distinctive methodology reject the import of narratives. But for those that do reject the import of narratives, this approach is regarded as orthogonal and of no use to those interested in epistemic matters. Of the philosophers of science cited above, Wylie, Chapman and Currie stand out as philosophers who think that narrative plays an essential role in the reasoning required to reconstruct the past. They are not alone in so thinking, for there are several other notable philosophers of science whose work takes seriously the import of historicity and of narrative (see, e.g. Beatty (2017); Beatty & Desjardins (2009); Beatty (forthcoming); Desjardins (2011a,b, 2015); Ereshefsky (2010); Ereshefsky & Turner (2019); Hull (1975)). Along with these philosophers of science, there are historians who are engaged in understanding the narrative form in scientific reasoning as well (see, e.g., Morgan (2017); Roth (2017a); Wise (2017)). Indeed, those taking narratives seriously reject the claim that issues pertaining to narratives are unrelated to issues of epistemology. It is clear that narratives play an important role in organizing information and in making history coherent, for example. It is also clear that narratives crucially involve turning points, that seem to violate the law-like prescriptions of phenomena that are encoded by theories in more traditionally conceived areas of science. In this sense, narratives capture a crucial difference between historical and ahistorical work and are indispensable to historical work. What remains to be seen is how narratives can be defended against the kind of dismissiveness displayed by the Cleland quotation above.

Finally, the fourth motivation to understand historical sciences is perhaps the most exciting.

There is a growing sense in the physics community that fundamental physics is in the midst of a crisis. Moreover, this crisis is thought by many to be methodological. The primary force driving this sense of crisis is that fundamental physics seems to be fast approaching the point where no new physics is accessible. The Large Hadron Collider has been something of a disappointment, insofar as it has not turned up any anomalies that are incompatible with the Standard Model of particle physics (for a compelling and relatively non-technical discussion of this, see, e.g. Hossenfelder (2018)). Physicists have many reasons for believing that our best theories are fundamentally flawed, and yet the competitors for alternative theories do not make predictions that can be verified by experiment because doing so would require, for example, particle accelerators that dwarf the Large Hadron Collider. String theories, as well as alternatives for more fundamental physics seem, if not in principle, practically untestable. Similarly, in spacetime physics, we know that General Relativity is incompatible with quantum theory, and yet probing nature at scales that would be revealing for the purposes of developing and testing theories of quantum gravity likewise seems not possible. This situation has led to the sense of crisis, and some philosophers and scientists have started to consider the fact that there are a great many types of scientific inquiry that seem to have much success and yet do not conform to the experimental method. Perhaps, it is thought, something can be learned from the approach taken by historical scientists, insofar as historical scientists generate scientific knowledge in spite of the fact that they cannot extract predictions from their theories and then go about experimentally verifying them. Dawid (2013), for example, thinks that string theory is confirmed in the same way that theories in paleontology are, viz. by what he calls forms of non-empirical confirmation. Unger & Smolin (2014) go a step further, arguing that physics needs to adopt what might be called a historical methodology, and search for global historical laws, that govern the Universe as a whole and how it evolves. Interestingly, then, physicists are starting to realize that historical scientists are legitimately called scientists and that, in fact, their approach to methodology might well be what is needed in physics.

This turn of the table is truly exciting, for insofar as this crisis seems methodological, there

is a very real opportunity for philosophers of science to make a genuine contribution. I hope to make such a contribution in what follows. My aim is to answer the following questions. What does it mean to call a scientific investigation a “historical science?” Does the temporal location of an investigation’s target really force scientists to pursue an alternative methodology? What should we make of the narrative structure of theories concerning the past, what epistemic role does this narrative structure play, if any? Is the narrative structure simply a heuristic or even pedagogical tool for the conveyance of independent research findings? If, as Cleland claims, coherence plays a role in this narrative structure (and I think it does), how is the “just-so” charge avoided, is there any way that coherence can contribute to justification?

So far, I have laid out the broad philosophical terrain concerning historical science. Critical discussion of these four different views of historical science is woven throughout the remainder of this work. The over-arching theme of this work is the exploration of a fifth approach. I question the distinction between historical and experimental science. I argue that the *prima facie* difference is due to an inadequate understanding of the methodology in both kinds of investigation. I also argue that the narrative structure is not only indispensable for the organization of research findings concerning the deep past, but that it plays an essential epistemic role akin to the role played by theories in physics. There are some differences between sciences that reconstruct the past and those that do not, which I will discuss, but a more nuanced understanding of how scientific claims are tested, I will argue, shows that these differences do not imply a difference in inferential methodology. Moreover, I will argue the converse of the claim that historical sciences are epistemically feeble insofar as they seek only to characterize token event. It may be true that historical scientists do not seek historical laws, but this misses the crucial role that historical investigations can play in ongoing ahistorical research. Indeed, many physicists have come to see that the study of the early history of the Universe is not at all an attempt to piece together a “quasi-fictional” story. Rather, the use of our best theories in physics to infer what happened 13 billion years ago and what is happening still constitutes an important test of our most fundamental physical theories, a test, it seems to Smolin and some



others, these theories are failing. A crucial test of our best theories in physics is accomplished not by way of careful, controlled experiment, but by way of putting them to use in the study of the Universe's history. The study of history, in other words, is the testing ground for theories, placing strong constraints on theories of quantum gravity as well as alternatives to the standard model of particle physics.

In §1.2, I explicate what I'll call the received view, that investigations of the past must employ an alternative methodology and describe its motivation. In §1.6, I show that, contrary to the received view, the epistemic situation faced by scientists investigating the past is not distinctive, and so the category "historical science" is ill-conceived. The proper distinction is between sciences that tell stories of the unfolding of events—I call these "story sciences"—and those that do not. I further show that the category of story sciences can be further subdivided into those investigations that produce story-sketches and those that produce full-blown stories. That a scientific investigation is focused on a target that is in the past, moreover, is not sufficient to guarantee membership in the class of story sciences. In §1.3, I address the argument first offered by Cleland (2001) and further refined and defended by Currie (2018), that the present overdetermines the past. I show that this thesis of overdetermination is based on problematic metaphysics—it is introduced by the (faulty) way in which Cleland and Currie (following Lewis (1979)) analyze counterfactuals. Moreover, the overdetermination thesis is based on an underlying and problematic assumption that we infer the past by way of retrodiction.

In Chapter 2, I argue that the presumption of a distinctive methodology is based on an impoverished understanding of what strong evidence is in both ahistorical and historical scientific investigations. I argue that the gold standard of evidence in justifying theories in more familiar areas of science is not prediction followed by independent verification, but, instead, consists in a theory being presupposed in order to refine precision of measurements and to discover additional finer-grained causal details of the world. I then explicate what I call story-mediated measurement, wherein a story of a singular event can play this same role in licensing ongoing measurements. I illustrate story-mediated measurement by showing how the story of the origin

of the Moon plays a constitutive role in licensing high precision measurements of when the Moon-forming giant impact occurred.

Chapter 3 is concerned with developing further the notion of story that I introduce in Ch.2. I argue, in agreement with the the third camp above, that the narrative structure is essential, that it is not merely a rhetorical device. I then show that my deflated notion of story resolves the problem of truth-aptness of narratives. The chapter then shows how stories do more than license measurements, they individuate the very lines of evidence that that the story is about. This then raises a potential circularity problem. How can a story be confirmed by evidence that is individuated by presupposing the very story the evidence is supposed to be evidence for? My answer is that a form of coherence is plays a crucial role that the story-mediated method developed in Ch.2 illuminates. Drawing on work in formal epistemology, I argue that coherence is a problem for views that emphasize explanation as playing a confirmatory role. Instead, what does the job of confirming stories concerning the past is that stories make disparate, non-independent lines of evidence mutually informative. The chapter then closes with a discussion that pushes the case study of the origin of the Moon further. Whereas Ch.2 was concerned with showing how the story played a constitutive role in using a story-independent isotopic chronometer to calibrate a story-dependent chronometer, here I will show how story-dependent chronometers can be used in order to re-calibrate story-independent chronometers. Story-mediated reasoning, in other words, is not the lesser cousin of story-independent law-like relations, but can be successfully used to refine them. And what justifies this practice is the mutual informativeness that stories exhibit. Lines of evidence have significantly more probative value when unified by a story than they do independently of it.

Chapter 4 is focused more directly to cases. Part 1 seeks to further illustrate the framework developed in the first three chapters by showing the remarkable progress that scientists have made in understanding the earliest history of the Earth-Moon system. In Part 2 I shift my focus to early Universe cosmology. There I address the issues raised by camp four above. I put the framework developed to use in answering the question, on behalf of cosmologists, what it

means for cosmology to be a historical science.

Before beginning, I wish to flag a reservation I have with terms like “historical science” and “experimental science.” The problem I have is not that I don’t think there are different types of investigation. I think there are many. The growth of work on mechanism and mechanistic explanation is an example. There are clearly scientific investigations that seek to understand part-whole relations. Analysis of the potential distinctive evidential challenges faced by different types of inquiry and what techniques are used to meet them is a useful and fruitful philosophical endeavor. Rather, I don’t like the distinction between historical and experimental for four reasons: 1) the contrast is too stark. The distinction is, at best, blurry. Almost every domain of science involves some investigations of history where they apply what they know on the basis of experimental reasoning. 2) This choice of terms already presupposes a distinction in method that I don’t think has been adequately subjected to philosophical scrutiny. 3) The distinction presupposes that we have a clear and adequate characterization of the experimental side. And 4) Making claims regarding the epistemic standing, say, of historical sciences as camp one above does, makes about as much sense as making such claims about whole domains of science. Epistemic standing is a local matter. My preference is for a term like “scientific investigations of the past,” but insofar as there is already a literature that I’m engaging with and the fact that my preferred term gets a little cumbersome, in what follows I will continue to use “historical science.”

## 1.2 Received View

What I’ll call the received view (associated with the second of the four approaches to historical science discussed above) is a response to a form of skepticism concerning the possibility of scientifically investigating the past. According to this skeptical view, investigations of the past characterize unpredictable and unique particulars, as opposed to regularities of nature. As such, historical investigations are concerned with a different kind of phenomenon, and an especially

dodgy kind at that. This is the basis of a mostly tacit belief among more laboratory-minded scientists and philosophers that there is a difference in the kind and quality of evidence that these sciences can garner, possibly a difference in methodology, and, thereby, *prima facie* epistemic inferiority. The worry is that investigations of the past infer past causes based on limited traces (distal effects) that they are lucky enough to find. Because the phenomena of interest are unique particulars, no new evidence can be generated, and, in fact, the ravages of time tend to erase most of the traces that were made to begin with. Both the uniqueness of the target and the loss of traces amount to additional epistemic hurdles that investigators of history face. Moreover, according to the (often Popperian) understanding of scientific testing that is not uncommon, these additional hurdles preclude the proper testing of theories and hypotheses.

But, according to the received view, we should not be as pessimistic as all of that. The skeptics are right, there is an alternative methodology. This distinctive methodology, however, negates the putative epistemic inferiority. The most prominent statement of this view is due to Carol Cleland (Cleland (2001, 2002, 2011)). In what follows, I'll start with an exposition of Cleland's influential view. I'll then describe Adrian Curry's recent refinements of the view. I'll then proceed to critical engagement in which I will criticize the putative metaphysical underpinnings of the view as well as undermine the motivation for it. This Chapter is, therefore, mostly negative, with my positive view coming in Ch.2.

Cleland's account begins with an articulation of the method employed by experimental scientists. Her account of the experimental method, moreover, is a refinement of the Popperian picture many scientists explicitly endorse. According to Cleland: "Hypotheses investigated in classical experimental science postulate regularities among event types. A test condition C is inferred from the hypothesis [coupled with auxiliary assumptions] and a prediction is made about what should happen if C is realized (and the hypothesis is true). This forms the basis for a series of controlled experiments (Cleland, 2002, p.476)." Cleland then goes on to stress how these controlled experiments seek to safeguard against false positives and false negatives. Scientists manipulate the experimental apparatus as well as vary the test condition C in order

to make sure that the predicted outcome is actually the result of the test condition and not some other accidental feature of the setup. They also, in the face of negative results, conduct further controlled trials in order to test whether the negative result is due to faulty equipment or some other issue with the slew of auxiliary hypotheses. In so doing, scientists are “engaging in systematic, extended experimentation that sometimes resembles an attempt to falsify a hypothesis and sometimes resembles an attempt to protect a hypothesis from falsification, but is really aimed as something quite different, namely, minimizing the very real possibility of misleading confirmations *and* disconfirmations in concrete laboratory settings. The historical tendency of philosophers to take the isolated experiments as the descriptive unit of experimental research has obscured this character of classical experimental science ((Cleland, 2002, p.478)).” What Cleland is stressing here is the familiar point made by Duhem (1991), that failure of prediction to match experimental outcome falsifies not the hypothesis but the combination of hypothesis plus the myriad other auxiliary assumptions. Proper falsification of a hypothesis requires ruling out many alternative possibilities and failing to do so is to declare defeat too soon. On the other hand, agreement between prediction and outcome can be due to accidental features that have nothing whatsoever to do with the hypothesis, and so, again, scientists are—or at least ought to be—careful about not declaring victory too soon. In my view, this is an important insight and worth stressing. I will come back to this idea below.

Insofar as the target phenomena of historical science is unique, often large scale events, they clearly cannot test their hypotheses in anything like the way that classical experimentalists can. Cleland characterizes the inferential method thus:

In the prototypical scenario, an investigator observes puzzling traces (effects) of long-past events. Hypotheses are formulated to explain them. The hypotheses explain the traces by postulating a common cause for them. Thus the hypotheses of prototypical historical science differ from those of classical experimental science insofar as they are concerned with event-tokens instead of regularities among event-types. This helps explain the narrative character of many historical explana-

tions. The complexity of the causal conditions and the length of the causal chain (connecting the cause to its current traces) bury the regularities in a welter of contingencies. Accordingly, it is hardly surprising that historical explanations often have the character of stories that, lacking reference to specific generalizations, seem inherently untestable. Nonetheless, it would be a mistake to conclude that hypotheses about the remote past can't be "tested." Traces provide evidence for past events just as successful predictions provide evidence for the generalizations examined in the lab. Instead of inferring test implications from a target hypothesis and performing a series of experiments, historical scientists focus their attention on formulating mutually exclusive hypotheses and hunting for evidentiary traces to discriminate among them. The goal is to discover a "smoking gun." A smoking gun is a trace(s) that unambiguously discriminates one hypothesis from among a set of currently available hypotheses as providing "the best explanation" of the traces thus far observed. ((Cleland, 2002, p.481))

This needs a fair amount of unpacking. Cleland's view is that the aim of historical scientific hypotheses is to explain puzzling traces by postulating common-cause explanations. And these are "tested" by a kind of eliminative inference. The idea is essentially to formulate a set of alternative causes that could have given rise to the puzzling trace and then elaborate a list of all of the other traces each would have or could have produced. Any potential trace that is on the list of possible traces from one cause but not the others is a potential smoking gun, for, if found, it would eliminate the others from consideration. Moreover, the inference from extant traces to past causes is more or less direct, and the aim of hypothesizing events is to explain traces that are observed today (more on Cleland's insistence upon common cause explanations in a few paragraphs below). Often, however, the reasoning won't be quite so straightforward, for there might be more than one past cause that would equally explain the observed traces. In such situations, Cleland's prescription is that historical scientists should—and do—go out into the field and find a "smoking gun."

Cleland's favored example of a smoking gun is related to the case of the extinction of the dinosaurs. On Cleland's construal, scientists had found traces that were compatible with two hypothetical causes: 1. The dinosaurs appear to go extinct rather suddenly, about 65 million years ago. And, 2. there is a clearly resolvable excess of iridium in strata corresponding to this period. These two traces are insufficient to pick between massive volcanism (because there is also ample evidence of massive volcanic activity at the time from the Deccan traps in present day India) as the cause of both or impact by a large asteroid (see also (Keller et al., 2009, p.711), Pettinari et al. (2003); DePalma et al. (2019a). Both scenarios could explain the finding of sudden extinction and both are potential sources of the iridium layer. The job, then, was to go out and find another trace that is compatible with one but, not the other, scenario. And, according to Cleland, this is just what was done. Researchers went out and found that there is a distinctive pattern of shocked quartz, a substance that is only found in two kinds of places on Earth, in large meteorite impact craters and in nuclear weapon test sites. This trace, then, was a smoking gun, for volcanism cannot account for the presence of shocked quartz. This example illustrates another thing that is important for Cleland. In personal correspondence, Cleland stresses that evidential lines—and especially smoking guns—must be independent of each other and independent of the hypothesis. In the case of the extinction of the dinosaurs, for example, one need not know anything about the dinosaur extinction to go out and find the iridium layer, shocked quartz, etc. The analysis of these lines of evidence makes no presumptions as to the cause of the extinction event, nor does one need to assume anything regarding the other lines of evidence in order to measure or observe or characterize each line of evidence. Indeed, for Cleland, the picture is one where historical scientists pursue many independent lines of evidence and convergence on the same hypothesis constitutes a successful test of that hypothesis.

There are a few more ingredients necessary in order to arrive at Cleland's conclusion, viz. that this method of testing is epistemically on par with the method of testing in experimental work. Cleland still needs to address the following evidential challenges: 1) Some, in-

deed, many, processes destroy the traces left by other processes (Information Loss). 2) Traces overlap—they are not isolated—making traces ambiguous as to which past process they are genetically related to (Ambiguity of traces). And 3) Given 1) and 2), why should we expect that there are traces persisting to today that are sufficient for us to reliably infer their cause (Insufficiency of traces)?

As is clear from the quotation above, Cleland thinks that common cause explanation plays a crucial role. Recalling the lengthy Cleland quote from the previous section, she is concerned with narrative explanations that rely on appeals to coherence for confirmation or disconfirmation of historical hypotheses. Instead, Cleland argues that common cause explanation does the needed confirmational work. Cleland further claims:

Common cause explanation promises a solution to the problem of evidential warrant faced by narrative explanations in natural science. The basic idea behind common cause explanation is to formulate reliable inferential methods for identifying when a diversity of contemporary traces comprises the effects of a common cause token. . .

Common cause accounts of explanation are traditionally justified by appealing to the principle of the common cause. The principle of the common cause is associated with the work of Reichenbach (1956). . . I take the principle of the common cause to (roughly speaking) assert that seemingly improbable coincidences (correlations or similarities among events or states) are best explained by reference to a shared common cause. The principle of the common cause represents an epistemological conjecture about the conditions under which a certain pattern of causation may be non-deductively inferred. According to the principle of the common cause, most seemingly improbable coincidences are produced by common causes.

The principle of the common cause presupposes an ostensibly metaphysical claim about the temporal structure of causal relations among events in our universe. Genuinely improbable coincidences are rare. Most otherwise improbable coincidences



are produced by common causes. In the next section, I argue that this supposition is not merely metaphysical. ((Cleland, 2011, p.17–18)).”

The idea seems to be this. Recall that the method involves enumerating possible alternative common cause explanations for some puzzling traces, and identifying the sorts of traces that we might expect would have been produced under each scenario, with potential smoking guns being traces that are on one list but no others. The principle that justifies this kind of eliminative reasoning is that, in the absence of a common cause, the puzzling traces found so far are highly improbable. But, Cleland notes, this principle stands in need of empirical justification and, she thinks, it can be so grounded.

I now turn to Cleland’s argument that appeal to common cause explanation as providing confirmational boost is not merely metaphysical. The hope is that the argument will address the three evidential challenges raised a few paragraphs ago. If that argument does not work, then common cause explanations will be in no better standing than narrative explanations, which, according to Cleland, depend for their epistemic standing on vague notions of coherence and explanatory power.

### **1.3 Can the asymmetry of overdetermination thesis save the day?**

Appeal to common cause explanation for epistemic justification is not without its problems (see, for example, Sober (1988, 2001); Turner (2005)). One pressing issue is that the principle of the common cause stands in need of its own grounding or justification. Aware of this, Cleland argues for a new grounding for the principle. Whereas other attempts to do so simply appeal to further metaphysical claims that, themselves, stand in need of justification, Cleland argues that the “use of the principle of the common cause by historical natural scientists rests upon a substantive thesis about the nature of the world for which there exists overwhelming empirical evidence, namely, the thesis of the asymmetry of overdetermination ((Cleland, 2002,

570)).” The asymmetry of overdetermination serves two purposes for Cleland. It gives empirical justification for common cause reasoning. It also justifies optimism that, despite the additional epistemic hurdles inherent in studying the past, we should be optimistic about the ability of historical scientists to produce genuine knowledge of the deep past by smoking-gun-style reasoning. The asymmetry of overdetermination insures that not all traces from some past event are necessary for inferring what happened so that smoking-gun-style inferences are (or can be when nature is generous) on epistemic par with experimental scientific inferences.

The asymmetry of overdetermination is a claim about how causes and effects are asymmetrically connected in time. The present state of a system, say an experimental setup, does not determine the outcome, even if the setup is prepared in accord with known dynamical laws. The outcome of the experiment can be circumvented in any number of ways, and so the future state of a system is underdetermined by the present. For example, an experimental setup can be prepared such that when the apparatus is switched on, some predicted outcome will invariably follow. But that outcome may, in fact, not occur in virtue of my failing to turn on the apparatus, or the building might fall down, the apparatus might malfunction, etc. This is one of the reasons it is so important for experimentalists to not declare defeat too soon, for one of the necessary auxiliary assumptions of any experiment is that everything works properly.

The future, in other words, is open; it is underdetermined by the present. The present, on the other hand, overdetermines the past. The idea is that causes generally have many and widespread effects or traces such that not all traces are necessary for inferring what happened. One doesn’t need every shard of glass from a window smashed by a brick; one needs only the brick and a few shards. Information loss, then, is not grounds for pessimism or skepticism regarding historical inquiry. “[The asymmetry of overdetermination] tells us that a strikingly small subcollection of traces is enough to substantially increase the probability that a past event occurred, and that there are likely to be many such subcollections. The existence of so many different reliable possibilities of identifying past events provides the rationale for the historical scientist’s emphasis on finding a smoking gun. . . One can never rule out the possibility of find-

ing a smoking gun, and this is a consequence of an objective fact about nature, namely, most past events are massively overdetermined by localized present phenomena ((Cleland, 2002, p.491–2)).

Such is the claim, now for the argument for it. The argument is due to David Lewis (1979) and it turns on yet another asymmetry, the so called asymmetry of miracles. Lewis' concern was not with historical inquiry, *per se*. Rather, his aim was defending his preferred analysis of counterfactuals. For example, take the following counterfactual: Apparently Nixon came very close to pressing the nuclear button, but didn't. If Nixon had pressed the button, the world would have been very different. What Lewis wants is an analysis that yields the right truth conditions for counterfactuals—in this case, the counterfactual should come out true—and he wants it to exclude what he thinks is a change of subject, namely, that the counterfactual is evaluated in terms of how the world would have had to have been such that Nixon in fact pressed the button. Lewis calls the latter a backtracking counterfactual, which he wants to exclude because he thinks those are not the standard way in which counterfactuals are intended. I'll return to the issue of backtracking counterfactuals.

Lewis famously analyzed counterfactuals in terms of “possible worlds.” In order for a counterfactual to be true, it must be the case that those possible worlds in which the antecedent is true are the the possible worlds that are, in some sense, closest to the actual world. In the example, Lewis wants to know under what criteria the possible worlds in which Nixon pressed the button are close to the actual world. Let  $W_a$  be the actual world and  $W_1$  be a possible world that is exactly the same as  $W_a$  except that a tiny miracle occurs just when Nixon considers pressing the button, say a few different neurons are made to fire and so he presses the button. Since the counterfactual should come out true,  $W_1$  should be close to  $W_a$ , and so it is evident that close possible worlds can diverge radically after the event in question. But what about a world,  $W_2$ , in which Nixon presses the button but there's a tiny miracle, say one that induces a short circuit, and so nothing happens? If  $W_2$  counts as a close possible world, then the counterfactual comes out false, for  $W_2$  matches  $W_a$  in matters fact not only in the time prior

to the event, but thereafter as well, save for the tiny miracle. So Lewis' criteria for closeness of possible world must exclude  $W_2$ . Lewis does so by pointing out there is an asymmetry of miracles.

According to Lewis, there is actually a big difference between  $W_1$  and  $W_2$ , such that  $W_2$  is not nearly so close to  $W_a$  as it appears. The issue for Lewis comes down to the size of the miracle. It seems that the two miracles, one occurring before the event and one after, are similarly tiny. The problem is that  $W_2$  would diverge radically from  $W_a$ —albeit the divergence would be different from the way  $W_1$  diverges—and so the counterfactual comes out true. The reason for this that in order for the counterfactual to be false in  $W_2$ , the tiny (short-circuit-inducing) miracle is insufficient for the subsequent history of  $W_2$  to match  $W_a$ . “At least for a while,  $[W_a]$  and  $[W_2]$  remain very closely similar in matters of particular fact. But they are no longer exactly alike. The holocaust has been prevented, but Nixon's deed has left its mark on the world  $[W_2]$ . There are his finger prints on the button. Nixon is still trembling, wondering what went wrong—or right. The click of the button has been preserved on tape. Light waves that flew out of the window, bearing the image of Nixon's finger on the button, are still on their way into outer space...I should think that the close similarity between  $[W_a]$  and  $[W_2]$  could not last. Some of the little differences would give rise to bigger differences sooner or later. Maybe Nixon's memoirs are more sanctimonious at  $[W_2]$ . Consequently they have a different impact on the character of a few hundred out of the millions who read them. A few of these few hundred make different decisions at crucial moments of their lives—and we're off! (Lewis, 1979, p.45–46)” So the idea is that the tiny extra miracle in  $W_2$  actually does lead to a world that is very different from the actual world.

Alternatively, if  $W_2$  is to remain so similar to  $W_a$ , then the tiny extra miracle is not at all as tiny, for it would involve additional miracles, miracles required in order to erase all of the traces of Nixon's having pressed the button—Nixon's memory, his fingerprint, the light rays carrying his image into space, and so on. And not only that, but there would need to be miracles erasing each of these miracles, for the miracles would leave traces too, and so on.  $W_2$ , then, is similar

to  $W_a$  in matters of particular fact but only insofar as  $W_2$  is infected with a massive outbreak of lawlessness, i.e. miracles, and so  $W_2$  does not count as a close possible world.

The asymmetry of overdetermination now follows from the asymmetry of miracles. What we learn from the Nixon counterfactual analysis is that the asymmetry of miracles is really a fact about traces. In order for a possible world to radically diverge from the actual world in matters of particular fact, a tiny change prior to an event in question is all that is needed. But for a possible world to converge to the actual world from a different past requires a compounding set of miracles. A tiny preemptive miracle can radically change the future. But in order for the past to be different but the future the same, an enormous number of improbably (miraculous) things must occur. There is, then, essentially only one lawful way for the world to arrive at its present state. So the likelihood that all relevant traces from a past event are actually erased is extremely low. And so, in Lewis' words, "Whatever goes on leaves widespread and varied traces at future times. Most of these traces are so minute or so dispersed or so complicated that no human detective could ever read them; but no matter, so long as they exist. It is plausible that very many simultaneous disjoint combinations of traces of any present fact are determinants [a minimal set of conditions jointly sufficient, given the laws of nature, for the fact in question] thereof; there is not lawful way for the combination to have come about in the absence of the fact... If so, the abundance of future traces makes for a like abundance of future determinants. We may reasonably expect overdetermination toward the past on an altogether different scale from the occasional case of mild overdetermination toward the future. ((Lewis, 1979, p.50))"

So the argument, for Cleland, amounts to this: The asymmetry of miracles entails that there will always be some traces that persist and we don't need all of them. Moreover, a world in which there are traces that indicate some past occurrence but where that past occurrence didn't occur is a world in which the links between determinant(the traces) and determined (the past cause that the traces are sufficient to establish) are broken, and this requires miracles (in the form of violations of natural law) or postulating a great many improbable coincidences. This follows from the fact that, were the past different, the present would also have to be very

different. So, it seems, common cause explanations are far more probable, and though we may never find the right subcollection of traces that suffices, we are assured that they are out there and so we can never rule out the possibility of finding a smoking gun.

Moreover, as noted above, Cleland claims that the asymmetry of overdetermination justifies common cause reasoning. Cleland's argument on this point is unclear, but the following is my best attempt at a charitable formulation that is at least in the spirit of what Cleland intends. The asymmetry of miracles assures us that cover-ups are extremely difficult. Consider  $W_2$  from the Nixon case, the world in which a tiny miracle occurred after Nixon presses the button that prevents the coming nuclear holocaust. In that world, there would be abundant traces that suffice for Nixon's having not pressed the button. These traces would be, in some sense, false. But, as Lewis argued, these would not be the only traces in  $W_2$ . The asymmetry of miracles guarantees that future times sensitively depend upon earlier times, such that Nixon's having pressed the button would necessarily have many other effects—leave many other traces—that would in turn have other effects and so on, making the world very different. So, in addition to the abundant “false traces” that make it seem as though Nixon didn't press the button, there would be abundant traces that show that he did, and so his having pressed the button will be, nevertheless, overdetermined. So, the asymmetry of miracles justifies common cause reasoning because in order for there to be traces that do suffice for common cause inference and no traces that indicate something funny happened, the only lawful way for this to occur is that the common cause event occurred.

The method of testing employed in experimental work is appropriate because the future is underdetermined, and so scientists take advantage of the fact that they can manipulate and control phenomena in order to rule out false positives and false negatives. By contrast, historical scientists cannot conduct controlled manipulation, but this in no way diminishes their epistemic credentials, for they do not face the problem of underdetermination. To be sure, sometimes nature is ungenerous, and so smoking guns are sometimes incredibly hard to find. But we can be confident that they exist and, when found, they provide comparable evidential

value to controlled experiment.

Let me now sum this up: Cleland is worried about the view that historical science is epistemically inferior because historical scientists can't derive predictions from hypotheses and then conduct controlled experiments to verify them. This is due to their distinctive epistemic circumstance, that they must reason differently, from effects to causes. On the other hand, common cause explanations can be particularly successful and that sometimes a remarkably small number of traces is enough to eliminate alternative explanations. In spite of all of the traces that have been lost, just a handful of traces is sufficient to falsify alternative hypotheses in favor of one. Eliminative reasoning looks like a way to "test" common cause reasoning. But the principle of the common cause is a metaphysical claim, so if common cause explanations are to amount to knowledge, the principle stands in need of justification. More specifically, Cleland needs to show that improbable associations, viz. that sets of traces that make it look like something happened when it didn't, are in fact rare. And she also needs to argue that there will generally always be a sufficient number of traces to infer that something happened, that information loss due to the ravages of time won't make the situation hopeless. Fortunately for Cleland, David Lewis's asymmetry of overdetermination thesis provides just this, a substantive claim about a contingent feature of the world that supports both the claim that there are sufficient traces because not all traces can be erased and the claim that we don't need to worry too much about misleading traces.

Lewis, argues that particular facts about the world, past, present or future are determined and so have at least one determinant. Determinants of future facts are causes, whereas determinants of past causes are traces. He then uses the thought experiment involving possible worlds in which Nixon is considering pressing the button to launch a nuclear attack. What he notices is that it would only take a very tiny change to make Nixon press the button, but that after doing so, such a possible world would deviate radically from the actual world. But if we insert another tiny change, after Nixon presses the button, which prevents the nuclear holocaust, Lewis argues that the world would still radically deviate from the actual world, though

in a very different way. Because the future so sensitively depends upon the past, once an event occurs, all traces of it cannot be destroyed without generating tons of new traces to the effect that the other traces were destroyed and so on. To erase all traces would require a gigantic cover-up operation that would, itself leave traces that radiate out and would easily be detected. And so improbable associations are of little concern (they amount to something of a cover-up), and information loss threatening our epistemic aims is likewise thwarted because erasing all traces would leave traces, etc.

For Cleland, this amounts to a very nice justification of smoking guns and common cause reasoning.

## 1.4 Currie's Refinements

The above argument for overdetermination, and that it provides empirical justification for common cause reasoning, doesn't work. I've done my best to motivate it and show how Cleland and Lewis arrive at their conclusions, but the fact is there is a problem with the way in which the argument slides from what looks like an ontic claim, a claim about the way the world is, to a claim about what we can know about the world. Moreover, it is not clear what the ontic claim, that the present overdetermines the past, amounts to; what is the nature of this overdetermination? It can't be causal, but neither Cleland nor Lewis tell us what the relation between past and present is such that we can evaluate whether the relation is overdetermined. The argument assumes that, from the claim that not all traces of a past event can be destroyed, it follows that there are extant subcollections of traces that are sufficient to establish what happened and that we can, therefore, use these facts to empirically test hypotheses. But the latter simply does not follow from the former. Referring back to the three evidential challenges I described above, Cleland's argument purports to address the first, information loss, and the third, insufficiency of traces. If the asymmetry of miracles argument works (which I will argue it doesn't in the following section), the information loss issue is addressed. But there is a gap in the argu-



ment insofar as it follows from this neither that the traces that are extant amount to a sufficient subcollection, nor that we can know this.

I am not the first to point this out (Forber & Griffith (2011); Turner (2005); Currie (2018). Currie (2018) has done much to refine Cleland's argument and to make it more plausible. Insofar as I think even Currie's refinements do not save the argument, I will not dwell here long. But before giving my criticism in the next section, it's worth a moment developing the most plausible version of the argument for overdetermination.

Currie doesn't use this language, but I think it is consistent with his view that there is something of an infelicity in the term "asymmetry of overdetermination." Overdetermination is usually discussed in the context of causation. If both my children throw rocks of sufficient weight at a window, the window's breaking is overdetermined in the sense that had one of them failed to hit the window, the window still would have been broken. And Cleland's borrowing the argument for overdetermination from Lewis makes it seem as though this is the kind of overdetermination she means, since Lewis's work on the semantics of counterfactuals was an offshoot of his work on the metaphysics of causation. To be fair, Cleland has always been clear that this is not a metaphysical claim, but an epistemic one. The problem that makes this issue difficult is that Cleland's epistemic claim is supposed to follow from Lewis' metaphysical claim concerning the overdetermination of miracle. Indeed, as the quotations above show, it's plausible that Lewis also thought that the epistemic claim follows from the metaphysical. What's needed in order to refine the argument is to provide a relation that goes from traces to their causes that is not causal and Currie does just this.

(Currie, 2018, p.88) notes that in order to make better sense of the overdetermination thesis, what's needed is a non-causal, but still ontic relation that holds between present states (traces), say  $S_1$ ,  $S_2$  and  $S_3$ , and some event  $P$  (past cause). Currie proposes the relation he dubs "minimal dependence," which is a relation that holds between two variables when small changes to one variable make it likely that the other variable will be different as well. In this case, small changes to  $P$  make it likely that the  $S$ 's will be different. Likewise, if the  $S$ 's were different,

$P$  would likely have been different.  $P$  is necessary for the  $S$ 's, but does not guarantee their existence as they could be overwritten by intervening events. Likewise, from this it follows that the  $S$ 's are sufficient for  $P$ . Now suppose some intervening event occurs such that  $S_1$  is lost. According to Curry, " $S_2$  and  $S_3$  would still occur and moreover still guarantee  $P$ 's having occurred. That is, because each state is alone sufficient to guarantee  $P$ 's occurrence,  $S_1$ ,  $S_2$  and  $S_3$  overdetermine  $P$ ."

So what Currie has managed to do is show how to get the asymmetry of overdetermination from the asymmetry of miracles. The asymmetry of miracles establishes that not all traces can be destroyed by intervening processes, or at least they can't be without leaving other traces that would enable us to infer that those processes occurred, and so forth. Couple this with the ontic minimal dependence relation that holds between past and present, and a few traces will be more than sufficient. The upshot is that ontic overdetermination yields evidential redundancy.

But, as Currie notes, the epistemic claim, that evidential redundancy yields the desired epistemic goods is problematic. Currie's ontic formulation clarifies the nature of the overdetermination thesis. The occurrence of each of the  $S$ 's depends upon the prior occurrence of  $P$ . If intervening processes erase some of the  $S$ 's, so long as one of them survives then there is some state of the world that necessitates that  $P$  occurred. There is still an epistemic gap, however. Each  $S$  is sufficient to *know* that  $P$  occurred only if we have what Currie calls an appropriate midrange theory. A midrange theory is simply a theory that tells us that when  $P$  occurs, some  $S$  is produced. Note, too, that different types of  $S$  may be informative of  $P$  in conjunction with different midrange theories. So it may well be the case that we are literally surrounded by traces that are mutually massively overdetermine that  $P$  occurred, and yet we might still fail to know that  $P$  did because we are ignorant of the requisite midrange theory(ies). The evidential redundancy secured by ontic overdetermination and the asymmetry of miracles does not support the epistemic claim that Cleland wants.

For Currie's purposes, this is OK. His aim is more modest than Cleland's, as Currie is arguing for optimism concerning historical scientists' prospects for addressing what look to be

tougher evidential and epistemic circumstances. While the epistemic claim does not follow from the evidential redundancy, the latter suffices to motivate optimism and continued work in developing the appropriate midrange theories and the hunt for more traces. Moreover, Curry goes on to argue for what he calls the “Ripple Model of Evidence.” This model of evidence is supposed to convince us that, while it is possible for the traces of an event in the past to be so attenuated by the intervening processes, we should not expect this to be the norm. That is, Currie’s argument for the ripple model of evidence presupposes the the ontic notion of overdetermination and the asymmetry of miracles. According to the model, there are competing trends that tend to wash out. One trend is information loss; traces tend to be lost due to subsequent goings on. This epistemic loss, though, is made up for by what he calls “dispersal.” Dispersal is an application of the idea behind the asymmetry of miracles. Not only, according to the asymmetry of miracles, is it the case that all traces from an event cannot be erased. Recall that the reason that this is so is that a tiny change in a system has effects that radiate out in many different ways; causes never have just one effect, they have many. And these effects then generate other effects, such that with the passage of time, the number of effects owing to the initial cause grow and grow, as ripples radiating out from a disturbance in a pond. So, given the right kinds of midrange theories, we should be optimistic that cases where the trace base is so diminished that we cannot infer what happened will be limited.

I’m not going to go any further into Currie’s view of evidence. For my purposes, it is enough to have shown that Currie’s argument builds from his refined ontic conception of overdetermination and that this presupposes the asymmetry of miracles. In the next section I’m going to argue that the asymmetry of miracles is false, and so, therefore, is the asymmetry of overdetermination. What we should expect, instead, is that most of the time the present state of a system underdetermines the past, the present is compatible with a large class of possible past states.

## 1.5 Against Overdetermination and the Received View

In the previous section, I planted a seed, so to speak, that I now wish to bring to fruition. The seed was the second of the three evidential challenges that historical scientists face. Both Curie and Cleland tried to address the first—information loss—and the third—insufficiency of traces. Neither addresses the ambiguity of traces. This problem is simply the observation that all the traces from one cause are mixed up in a complex tangle with the traces from other effects. Sometimes this leads to traces being lost, i.e. information loss, but sometimes they are only modified. So while I don't doubt that most causes have varied and widespread effects, the problem is that these effects are mixed up with the effects of countless other causes and it is often very unclear which effects belong to which causes. Moreover, because traces are never isolated, which aspects of the traces we find today are due to the past cause that is the target of investigation and which are due to the processes that served to preserve the traces and which are due to other intervening causes?

In order for Currie's refined ontic conception of overdetermination to work, at least four things are needed. As Currie notes, we need a midrange theory that tells us what effects an event would have produced. But in addition to this, we need to know of some particular trace, that it was produced by the cause in question and not some other instance. Everything is a trace from some cause or other, and though many events are unique, the kinds of traces they produce are typically not. So to say, in Currie's terminology above, that some downstream state  $S_1$  requires that some past cause  $P$  occurred assumes either that there was only one  $P$  ever, or that we somehow know that this particular  $S$  derives from that particular  $P$ . It's plausible that some causes really are unique, and so some of their effects be unambiguous as to their cause, but this should be rare.

Additionally, what is needed for some  $S$  to be sufficient for  $P$ —to make it the case that  $S$ 's existence requires that  $P$  occurred— $S$  cannot be generated by some other process. That is, an  $S$  suffices for its cause  $P$  only if there is only one way to produce  $S$ . There needs to be only one lawful way for  $S$  to come about. This is probably not the case for very many single

traces. On the other hand, sets of traces probably can be sufficient. Cleland's discussion of the end of Cretaceous asteroid impact strikes me as one. The iridium layer does not require that an asteroid impact occurred, for it could have been produced by volcanism. The shocked quartz, likewise, has more than one possible source (shocked quartz can be produced by large meteorite impacts as well as by nuclear explosions). Together, they still do not, then, suffice to require that an asteroid impact occurred. Granted that nuclear explosion is incredibly unlikely, the point is that, logically, the two traces together do not suffice for the asteroid impact without some further knowledge. According to the overdetermination thesis, there should be many  $S$ 's that are independently sufficient for  $P$ . But because there are generally more than one possible way to produce any  $S$ , they are insufficient for any particular  $P$ . The best case scenario is that sometimes the total set of traces that we have is jointly sufficient for some past cause, but this is hardly a case of overdetermination.

The fourth additional thing needed concerns a distinction that I will discuss more fully in §3.4. Briefly, the issue concerns the fact that traces are not isolated insofar as, in addition to being mixed up together with traces from other causes, they are also changed by intervening processes. Extant traces have survived to the present typically by being preserved in some way. But preservation alters them. Some processes reliably alter traces, and so it is less difficult to determine what the effect actually was. But often, traces have been modified by countless intervening processes, making it unclear what their original state was. This makes their connection to the original generative process ambiguous.

This brief discussion of the ambiguity of traces has already gone some way toward undermining the overdetermination thesis. I now turn to showing that the asymmetry of miracles, on which the overdetermination thesis rests, is false. The argument is due to Elga (2001). Elga's argument is aimed at Lewis, not Cleland or Currie, but since both presuppose Lewis' asymmetry of miracles, the argument targets them as well.

Recall that Lewis was concerned with developing an account of counterfactuals. Schematically, for Lewis the counterfactual "If  $A$  were true,  $C$  would be true" is true if and only if  $C$

is true in the worlds in which A is true that are closest to the actual world. For this analysis to work, then, Lewis needs to specify the criteria for how to judge what the closest possible worlds are. Lewis then investigates some counterfactuals with seemingly obvious truth values in order to determine these criteria. Taking a counterfactuals like “Had Nixon pressed the button, the world would have been very different,” Lewis arrived at the following criteria: 1) “It is of the first importance to avoid big, widespread violations of [natural] law,” and 2) “it is of the second importance to maximize the spatio-temporal region throughout which perfect match of particular fact prevails.” (Lewis, 1979, p.47)

He arrived at these criteria by noticing that a world in which Nixon presses the button can share exact match with the actual world right up until Nixon considers pressing the button, and only a very tiny miracle is required to make the antecedent of the counterfactual true in that world. But, thereafter, the worlds diverge radically. Then he considered a second world, that again matches the actual world until the tiny miracle makes Nixon press the button, but then introduced a second tiny miracle just after Nixon pressed the button that prevents the signal triggering the nuclear holocaust. Comparing these two cases, the closeness of possible world criteria needed to pick out worlds like the first but not the second, for he already knew the answer he wanted, viz. that the counterfactual is true. And what Lewis argued was that, in fact, the second world would in fact diverge radically from the actual world, were it not for the introduction of an astonishingly large number of additional miracles. So even though the second world is one in which the counterfactual is false, this world is not the closest to the actual world. The closest world is the first, which has maximum spatio-temporal similarity and only a tiny violation of natural law, and since the counterfactual is true there, it is true. What Lewis’ asymmetry of miracles argument does is ensure that preventing something from happening by intervening before the fact requires only a tiny miracle, whereas intervening after the fact requires a huge miracle.

I switch now to Elga’s example counterfactual in place of Lewis’. The exposition of the argument is unnecessarily challenging for the Nixon example because in the actual world Nixon

didn't press the button. Elga uses the example of Greta, who cracked an egg into a hot frying pan at 8:00. The counterfactual to consider is "Had Greta not cracked the egg, at 8:05 there would not have been a cooked egg on the pan." Though the circumstances are going to get pretty weird, what Elga's argument shows is that, contrary to the asymmetry of miracles, intervening after the cracking of the egg does not require a huge miracle in order to make the counterfactual false.

What Elga does is show how to construct a possible world from the actual world by inserting a tiny miraculous intervention at 8:05 and then evolving the laws *backwards*. Consider the actual world at 8:05. If we run the tape backwards, we will observe what looks like strange behavior. The egg will slowly grow cooler, uncooking until 8:00 when it jumps off the pan and into the shell. The shell heals itself and Greta places it back into the refrigerator. Now, because of the way in which statistical mechanics works (for more detail see Elga (2001)), it would take only a tiny intervention on a very small portion of the phase space in order to make the backwards evolution from 8:05 to look the same as the forward evolution. This new possible world is gotten by making such a change, such that continuing to run the evolution backwards from 8:05, the egg just sits there continuing to cook. This is problematic for Lewis, for this world is a world in which the antecedent is true but the consequent false, and yet it satisfies the criteria for closeness of possible worlds—it contains only tiny violation of natural law and matches the actual world in matters of particular fact *after* 8:05, which shouldn't be possible.

To bring this discussion back around to Currie and especially Cleland, consider this matter from the standpoint of traces:

- 1. At 8:05 the actual world contains traces of Greta's having cracked the egg.
- So immediately after 8:05,  $W_3$  [the possible world gotten by running the laws backward from 8:05] also contains those traces.

Lewis would then argue that:

- 3. Since  $W_3$  is a world in which Greta doesn't crack the egg, immediately before 8:05

$W_3$  does not contain those traces.

- 4. Therefore a large trace-manufacturing miracle occurs in  $W_3$  at 8:05.

But 3. and 4 are wrong. To be sure,  $W_3$  before 8:05 is an incredibly strange world. There's a very large spatio-temporal region that behaves thermodynamically very peculiarly. Because the history of  $W_3$  is gotten by running the laws in reverse. There's a cooked egg in Greta's pan that is more rotten the further back in time you go, but gets progressively less rotten as you approach 8:05, at which point it is freshly cooked. By 8:05 it appears that the egg had just recently been cracked on the pan and cooked, when, in fact, it was never raw and never cracked on the pan. Moreover, this is all in accord with the relevant natural laws. To be sure,  $W_3$ , at 8:05 contains a small spatio-temporal region infected by the change induced by the tiny miracle, and the size of that region grows dramatically as you go to earlier times. But this strangely behaving region, incredibly unlikely as it is, does not violate the natural laws.

According to (Elga, 2001, p.S324), "One might agree with Lewis that there is an asymmetry of overdetermination—that "very many simultaneous disjoint combinations of traces of any present fact are determinants thereof; there is no lawful way for the combination to have come about in the absence of the fact." But that's all wrong. It only takes a small miracle to make the difference between the actual world (a world in which those same traces are all highly misleading). In general, the existence of apparent traces of an event (together with the laws and together with the absence of evidence that those traces have been faked) falls far short of entailing that the event occurred."

The upshot of this is that, in the Greta example, Cleland would insist upon the only legitimate explanation for all of the traces found after 8:05 is the common cause explanation, that she cracked the egg at 8:00. Indeed, the justification of the principle of the common cause, for Cleland, is that the only lawful way for the traces of Greta's cracking the egg at 8:00 to be present later is if she in fact did so. Either there would be traces of the fakery that radiate out, and these couldn't all be erased<sup>1</sup>, or this is a case of massive lawlessness which is not possible.

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<sup>1</sup> And this assertion also presupposes the asymmetry of miracles



But, again, those claims are wrong. There are not traces of fakery because the traces aren't faked. They're extremely misleading, but not fake, and they are there by means of perfectly lawlike (though improbable) goings-on.

Admittedly, Elga's argument is bizarre, something seems "fishy." It seems strange to go to such lengths to construct such a possible world. The odds of that happening may as well be zero. The problem is that it is in-principle possible, and in-principle possibility was enough to ground the initial overdetermination thesis. So Elga's argument cannot be dismissed on the basis of its fishiness, for if it is, then by the same token the original argument for the asymmetry of overdetermination should be likewise rejected. Either way, we can dispense with it.

I wish to briefly note a very different sort of argument against the overdetermination thesis. This is an issue that I am still thinking about, and so will only sketch the way the argument might go. Recall that Lewis was concerned with excluding backtracking counterfactuals from the analysis. The issue here is that, according to Lewis, and this seems plausible, when we consider a counterfactual of the form "Had some event in the past been different, the present would be different" we generally have in mind holding the past fixed, introducing a slight tweak, and then considering how this slight change would change the present. He called this the "standard resolution" of counterfactuals. But counterfactuals are vague, and sometimes what we are interested in when considering them is the backtracking interpretation. In such cases one is then considering how the world would have had to have been different in order to make it the case that the relevant event occurred.

Here is an example adapted from Lewis (1979) to illustrate backtracking. Jim and Jack had a quarrel two days ago. Jack is still very angry. But had Jim asked Jack for help yesterday, Jack would have helped him. On Lewis' standard resolution, the analysis of the counterfactual holds the past fixed, and considers whether Jack would have helped him. On the backtracking resolution, though, one might say, "but wait a minute. Jim is a proud man, and if they had a fight two days ago, Jim never would have asked Jack for help yesterday." The reason that Lewis wanted to exclude backtracking is that strange things can occur in this context that do not bode

well for Lewis' program of understanding causation in terms of yet another asymmetry, the asymmetry of counterfactual dependence. For Lewis, the claim, say, *A* causes *B* and *C*, means that *B* and *C* counterfactually depend on *A*, but *A* does not counterfactually depend on *B* and *C*. This means, roughly, that if I intervene on *A* I, thereby, intervene on *B* and *C*, whereas if I intervene on *B* and *C*, *A* is unchanged. The reason that backtracking is problematic for this understanding of causation is that the wrong things are counterfactually dependent. For example, one might see that if *B* had not occurred, *A* didn't either, for *B* is a consequence of *A*. But then *C* wouldn't have occurred either, and so *C* counterfactually depends upon *B*. Consequently, on Lewis' analysis of causation, under the backtracking interpretation of causation, *B* causes *C* but that's just wrong.

So Lewis simply excludes backtracking from the analysis, by restricting his consideration to non-backtracking counterfactuals, i.e. ones that don't violate counterfactual dependence. And right here is precisely where the problem is, for in so doing, Lewis has put in the asymmetry of overdetermination by hand. Lewis purports to argue for the asymmetry of overdetermination, when in fact he has presupposed it. By excluding the consideration of what kind of alteration to the past would have been needed in order for the antecedent of a counterfactual to be true, Lewis restricts consideration to possible worlds in which the asymmetry of miracles is true, and so the asymmetry of overdetermination seems to follow. This is problematic for two reasons: 1) the argument of the asymmetry of overdetermination (on which Cleland and Currie rely) is circular. And, 2) the argument excludes from consideration a crucial issue that investigations of the past are concerned with, viz. what are other possible ways in which the present could be the same but the past different.

I now turn to considering what I think is an important problem with the received view concerning the notion of explanation. There is an underlying problem that even authors who disagree who do not accept the overdetermination thesis, such as (Anderl, (2018; Tucker, 2004; Turner, 2005; Jeffares, 2010; Kosso, 2001; Sober, 1991), all seem to share. The common assumption is that the goal of reconstructions of the past is to explain the traces that we find

today. I do not share this assumption, for the only way that I can see to motivate it is with an underlying picture of hypothesis testing that is essentially hypothetico-deductive. Such a picture, I claim, is behind the now-common view that underdetermination of theory by evidence forces us to evaluate rival hypotheses by comparing their explanatory power, or parsimony, or some other theoretical virtue. As I will show in Ch.2, it is a mistake to think of scientists reconstructing the past as seeking to explain the available traces. It is true that one can craft an explanation of what we observe today given a theory of what happened in the past, but this is not the aim. The aim, rather, is generally to understand what happened by determining what factors made a difference to the final outcome and what differences they made.<sup>2</sup>

The view that the aim of reconstructing the past is to explain the present contributes to the sense that narrative explanations are epistemically deficient and so what's needed is an appeal to metaphysics to justify our epistemology. The problem is actually worse than Cleland's comment above concerning the length of the causal chain that buries regularities in a welter of contingencies. The problem with the view that the aim is to prognosticate and so explain the present is that there is nothing to appeal to for evidential confirmation. To appeal to explanatory power as confirmatory is to double count evidence. The lines of evidence for a story concerning the past are compatible with the story. But this compatibility does not license any boost in credence in the story, for of course they are compatible, the story was built in order to explain them. Take any story concerning the past, common cause or otherwise. Now ask what reason there is to boost our credence in the story. The aim is to have a story that is consistent with all of the lines of evidence. But then, when thinking about confirmation, the problem is that consistency does not boost credence.<sup>3</sup> Consistency is an important test of a story, but consistency alone does not boost credence. Consistency checks can certainly boost a story or hypothesis' promise, but to boost credence on this basis problematic. So if the received view is right, that the aim is to explain the present, then there are just no resources for confirming

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<sup>2</sup>Note, however, that by this I do not mean to suggest that they are not explanatory or that explanatory considerations are of no import. Rather, I mean only here to suggest that, insofar as one is concerned with testing and confirmation, explanatory power is insufficient. See below, as well as Ch.3.

<sup>3</sup>This issue is strikingly similar to the problem of old evidence for Bayesian confirmation theory.

these explanations and so there had better be an alternative methodology that provides epistemic boost. I'll return to this issue in Ch.3, where I'll show that there is an epistemic resource available for confirmation of stories that does not depend upon their explanatory power.

For the sake of argument, let's suppose that either my reconstruction of Cleland or Currie is excessively uncharitable or otherwise incorrect and so I haven't shown that the overdetermination thesis is false. I want to briefly consider whether, in Cleland's favored case of the extinction of the dinosaurs, the view of method in terms of smoking guns and overdetermination is up to the challenge. For Cleland, this is a solid case of overdetermination and of the power of smoking guns. On her construal, there is no longer any question as to what happened to the dinosaurs. Indeed, in personal correspondence (2018), in reply to my insistence that the claim that the asteroid impact is the cause of the extinction event requires much paleontological evidence and work and which is still at a very early stage, Cleland replied "paleontologists simply have nothing to contribute on the matter."

What's needed, on my view (which will be spelled out in the chapters to come), is for the story to be fleshed out by story-dependent individuation of new lines of evidence that reveal further, finer grained details and break the event up into a sequence of subevents, and so on. The impact hypothesis must earn its keep by doing epistemic work. Moreover, my claim is that this is, in fact, what scientists do in practice in order to test their hypothetical stories. In order to make the causal link Cleland (and Alvarez) wants, an enormous amount of further work needs to be done and until such time as it has been done, the impact hypothesis offers a promising how-possible story but no more. To be sure, some of the necessary follow-on work has been done. The impact needs to be better understood and the dynamics of impact and its aftermath must be investigated. Much has been done in this regard, including understanding what effect the particular impact site had, how the impact transpired and what happened immediately following. Paleontologically, we need to know much more about why the dinosaurs were wiped out, but many other reptilian lineages survived. We need to know why aquatic and marine lineages were absolutely devastated, while many terrestrial flora and fauna survived.

It's unknown what factors made the differences for lineages that survived and those that didn't. Likewise it's unknown what the mechanism was that produced such massive carnage. To be sure, the asteroid impact finding leads to promising hypotheses, but these must be followed up and empirically investigated in order to establish that the impact was, in fact, the cause. As well, the role that the massive Deccan volcanism played must be better understood. Many of the lineages that did go extinct were probably on their way out anyway (see, e.g. Keller et al. (2009); Schoene et al. (2015) and for more philosophical discussion Forber & Griffith (2011); Bromham (2016)).

Scientists implicitly know this kind of follow-on work is not only necessary for establishing causal claims but also important for ongoing work. Indeed, as Alvarez himself claimed shortly after my discussion with Cleland mentioned above, "The most immediate effects of the terminal-Cretaceous Chicxulub impact, essential to understanding the global-scale environmental and biotic collapses that mark the CretaceousPaleogene extinction, are poorly resolved despite extensive previous work." (DePalma et al., 2019b) Scientists have long known that this situation was problematic, and the longer they went without being able to unravel the immediate effects and link them to timecapsules, the more worried they became that they could not rule out that the asteroid impact coincided with, but perhaps did not cause the mass-extinction. At long last, however, Alvarez and team have made progress, but using story-dependent reasoning<sup>4</sup> in order to reconstruct the timeline of subevents that immediately followed the impact and to tie this sequence to timecapsules. Other investigators have also made progress in understanding the aftermath of the asteroid impact by analyzing cores drilled from the impact site.<sup>5</sup> This kind of work, of requiring the story to do epistemic work in licensing further inferences that flesh out the story into further detail, is essential to protect against adoption of a how-possible story as a how-actual story, and scientists implicitly operate with this in mind.<sup>6</sup>

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<sup>4</sup>As developed in Chapters 2, 3, and 4.)

<sup>5</sup>See Gulick et al. (2019) and references therein.

<sup>6</sup>My point here is not that the needed follow-on work after the discoveries of the iridium layer and shocked quartz, and so on, has not been done. To be sure, much has been done and much is ongoing. For a few important examples, see Morgan et al. (2016); Schulte et al. (2010); Pope et al. (1997); Morgan et al. (2013). My point, rather, is that this is the kind of ongoing work that is necessary and Cleland's account of smoking guns not only

It seems, then, that the smoking gun picture of historical inference would have scientists declare victory too soon, content with suggestive or promising how-possible explanations and depriving themselves of the kind of strong empirical evidence that is the hallmark of good science. Scientists want to know what happened, in detail, and the asteroid impact hypothesis has only in subsequent work begun to deliver on this kind of detail. We want to know the story, not just one aspect. Did the asteroid impact trigger the extinction, or were they already going extinct, was the extinction causally overdetermined? We still do not know. This putative case of overdetermination shows, at best, that the knowledge that is overdetermined is very little. If overdetermination is what secures our epistemic access to the past, sciences that investigate the past are in deep trouble.

When describing several cases, Cleland points out how the community of scientists were unconvinced, even when smoking-gun type evidence had been found. The upshot of this brief discussion, and I will return to this issue in chapter 3.5 & 4, is that scientists know that the way to establish claims concerning the past is investigate ever finer details. And, indeed, Cleland sometimes mentions this.<sup>7</sup> The problem, as I see it, is that Cleland thinks this additional work is besides the point, that more is learned but that this does not contribute to the epistemic standing of the course-grained smoking gun inferences. And so her view would have investigations declare victory too early. The reason that Cleland thinks this is that the move to pursuing finer-grained details involves uncovering lines of evidence that are not independently known. For Cleland, independent lines of evidence are the highest standard, so further work on dependent lines of evidence—i.e. lines of evidence that are individuated by presupposing a story of what happened (I develop this further in Ch.2), don't have confirmational value.<sup>8</sup> On the view developed in the rest of this work, however, the development of dependent lines of evidence

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seems to render evidentially unnecessary but that Cleland really does so regard them.

<sup>7</sup>See, e.g., Cleland (2002)'s discussion of the discovery of plate tectonics or Cleland (2011)'s discussion of Schweitzer et al. (2005)'s work on T. Rex.

<sup>8</sup>This is why, for example, Cleland views the extinction of the dinosaurs as settled by the asteroid hypothesis. For Cleland, paleontologists have nothing to contribute to the matter (Cleland (personal correspondence)). Whatever paleontologists might say must either presuppose the asteroid impact or something else and so their evidence won't be properly independent. As such, they will at best develop a detailed quasi-fictional narrative.

is epistemically essential, for doing so forces conjectures about the past to do epistemic work. Until hypotheses concerning the past are put to work, they are idle and so may well be just-so stories.

This issue relates back to Cleland's insights concerning experimental science. Recall that Cleland (rightly) complained that "The historical tendency of philosophers to take the isolated experiment as the descriptive unit of experimental research has obscured the character of classical experimental science (Cleland, 2002, p.478)." I think Cleland is guilty of an analogous sin with respect to historical sciences. She has focused on cases of common cause reasoning in isolation and extrapolated from there, obscuring the fact that historical scientists are seldom content with stopping at such a coarse grain.

What I think is lurking behind the thesis of overdetermination and taking the search for smoking guns as the inferential method is a picture of inference and hypothesis testing that is hypothetico-deductive (HD). Cleland's formulation of experimental reasoning is certainly more refined than Popper's, but it still belies a commitment to HD. Moreover, this overly simplistic picture of scientific inference also pushes us to think of inferring the past in terms of retrodiction. Retrodiction is supposed to be the time-reverse of prediction. When we predict, we observe the state of a system and evolve the system forward in time, according to our understanding of the relevant dynamics that govern the system, to arrive at the predicted future state of the system. We then need only allow the system to evolve and see whether the actual future state of the system matches what we determined on the basis of our understanding of the relevant dynamics. And agreement between a theory or hypothesis' prediction and observation is the strongest form of evidence that we have, according to HD. Retrodiction is supposed to work the same way, except instead of running the dynamics forward in time, we reverse the signs on the relevant quantities, and evolve the system backward in time to infer what state the system was sometime in the past. If we wanted to know, for example, the past configuration of the solar system, we can simply note the present positions of the planets and their momenta, reverse the sign on time, and evolve the system in the same way we would if we wanted to

know the future configuration of the solar system. Cases where this is possible are interesting, for we can generate visualizations of their evolution—a movie, if you will—forward in time, or in reverse, unproblematically.

There are big problems with retrodiction. Most discussions of the problem of retrodiction involve systems that are governed by dynamics that are irreversible, that are not time symmetric. When the dynamics involve an arrow of time, the movie metaphor breaks down; we cannot generate a movie that shows the evolution backward in time, but only forward. Take, for example, a case where a gas is confined to a small region in the corner of a container. Predict the future state of the gas using statistical mechanics is straightforward. Making some idealizing assumptions, including some time-asymmetric components and restricting the admissible states of the component molecules to exclude improbable fine-tuned states that would evolve counter-intuitively to derive a master equation that describes the gas's approach to equilibrium.<sup>9</sup> And, of course, we could use the dynamics provided by statistical mechanics to produce a visualization, a movie, of how the gas particles will spread out to fill the container. Now, suppose that instead of making a prediction, we want to make a retrodiction. Examining the container, we see that the gas is homogeneously distributed. We now want to retrodict the prior state of the gas; we want to generate the time reverse of the movie made of the gas going toward equilibrium. Here we encounter the problem of retrodiction immediately. If we were to take the current state of the gas (i.e. how it is confined in the corner) and do the analog of reversing the sign of the velocities of the planets and derive a new master equation that defines the gas's subsequent evolution, the answer we would get would be the same as the answer for the forward in time process. The problem here is due to our epistemic limitations. A Laplacian Demon (a being who knows the exact locations and momenta of every particle and has unlimited time and computational power and who can, therefore, write down a time symmetric dynamical equation that governs the motion of every individual particle and then solve all  $10^{23}$  of them) could do it. But for epistemically limited beings like us, the present state of a

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<sup>9</sup>This was precisely the feature Elga took advantage of in generating his bizarre possible world.



system does not contain information sufficient to fix its past states. Retrodiction is possible, but we get the wrong answers. Systems that we are forced to treat Statistically, always evolve toward equilibrium, unless we already know what past state they were in and so put in by hand a fine-tuned initial state that leads away from equilibrium.

This problem is ubiquitous in a wide variety of systems. If I place a ball on a track at the top of an incline, I can predict the future state of the ball. But, finding the ball at the bottom of the hill, I do not have enough information to retrodict the ball's prior state. I might guess that the ball started at the top of the incline, in which case I could reconstruct in detail how the ball came to be at the bottom. But this is not how retrodiction is supposed to work. If I know that the ball was previously at the top, then I'm not retrodicting the trajectory of the ball, for I already know what happened. The present state of most systems, again, does not contain information to uniquely fix the initial state, let alone intervening states. Instead, the present at best determines some broad equivalence class of possible pasts.

Perhaps there is further information to be had, however. We might, for example, find that there is a mark on the ball that exactly matches a barrier at the end of the track that keeps the ball from rolling away. Such a mark on the ball would tell us that the ball had struck the end of the track at some point. This would not, however, tell us when the ball struck the end, nor would the mark tell us that the ball struck the end by rolling down the track. I may have accidentally (or, deviously) dropped the ball once onto the end piece of the track, for example. As mentioned above regarding the ambiguity of traces, relatively few marks have only one possible source. And the dent in the ball doesn't have a label on it telling what struck it and when. We might even be able to make a good estimate of the velocity of the ball when it hit the end, depending on the depth of the mark, say. But still, this would not be enough to tell whether the ball hit the barrier the instant just prior to us walking into the room or last week or whether it hit some other barrier. So again, the present state only picks out a large class of possible past histories.

Have I illegitimately restricted the scope of the kinds of observations we could be doing to

bear on the problem? The ball, rolling down the hill will surely have communicated kinetic energy to the molecules of air in the room. If we had really high precision measurements of the distribution of the air, we might be able to find reason to think that the ball had communicated just the same amount of energy to the air that it would had it rolled down the track. But this won't do, for we are now back to the problem that time-asymmetric dynamics brings; we would need to know the state of the molecular distribution prior to the event in question.

In this section I have argued that the asymmetry of overdetermination is false. Elga's shows that "false" traces are in in-principle possible, which undermines the asymmetry of miracles. The asymmetry of miracles underwrites both the optimistic view that not all traces can be erased as well as the claim that there are multiple subcollections of traces that are sufficient to guarantee some past event occurred (i.e. the asymmetry of overdetermination). I then considered more mundane examples that show that the ambiguity of traces means that subcollections of traces will generally fail to pick out a single past cause and to show that because traces are not isolated, given some set there is reason to worry that they all belong to the same past cause. So even if a set of traces is sufficient to pick out a unique cause, they may not all belong to the same cause and so the inference would be faulty. Then I showed that there is another way in which information about the past can be erased that does not require considering miracles or possible worlds. Equilibrium is the great eraser, erasing all traces of a system's past. Moreover, the downfall of the asymmetry of overdetermination undermines the appeal to the confirmatory power of common cause explanation, and so the smoking gun method fails to deliver on the promise that finding smoking guns constitutes the testing of hypotheses that is epistemically on par with experimental testing. I've also shown at least part of the worry concerning the epistemic authority of historical science—the worry that they need an alternative methodology—is due to thinking that the aim of historical science is to explain the present. Putting this point in Currie's terms, When the relevant "midrange theories" that connect present traces to past causes involve time asymmetric dynamics—and this is the norm—the present does not uniquely pick out, let alone overdetermine, the past; the present underdetermines the past.

I've argued many things in this section, so a summary is in order. This section undermines the asymmetry of overdetermination claim which, in §1.2 and 1.3 I showed underwrite Cleland's proposed alternative methodology for historical science and Currie's Ripple model of evidence and his argument for optimism. Although it is referred to customarily as a thesis, it really amounts to several claims. According to the refined version of the thesis, *P*, some past event or cause, has many effects and so leaves many traces (*S*'s). *P* determines each *S* in the sense that it is causally necessary for each. Conversely, each *S* is sufficient for *P* for *S* to exist, *P* had to have happened. Moreover, since each *S* is sufficient for *P*, we have evidential redundancy which is further safeguarded by the fact that for all traces to be erased a giant cover-up operation would be required, and this would leave its own traces and so we could discover that. Finally, the overdetermination thesis claims that improbably coincidence are rare, thus safeguarding common cause explanations.

In response to this, I showed that

- Arg. 1: the overdetermination thesis doesn't address an additional epistemic hurdle that is rather severe, viz. the ambiguity of traces; even if the thesis successfully address the other two epistemic hurdles, failure to address the ambiguity of traces robs it of the hoped for epistemic benefit.
- Arg. 2: Elga's argument that the asymmetry of overdetermination is false. Whereas Arg. 1 showed that the asymmetry of overdetermination was insufficient to overcome all of the hurdles necessary to achieve the desired ends, Arg 2. demonstrates that traces can in fact be misleading without violating natural laws and that it is possible for there to be every indication that *P* occurred and no additional traces indicating it didn't, even though it didn't.
- Arg. 3: shows that if the asymmetry of overdetermination fails to ground common cause explanation, that Cleland is right to worry that no adequate testing method can render confirmational support to hypotheses that purport to explain the present.

- Arg. 4: if Arg's 1-3 fail, the situation isn't much better, for the present state most systems of interest only suffices to uniquely pick out past states for a being with unlimited time and computational power who has perfect knowledge of the position and momenta of every particle.
- Conclusion: The asymmetry of overdetermination really is the linchpin of the most influential formulation of an alternative methodology for historical science, but it is, at best, epistemically useless for beings like us, but probably false.

What remains of this chapter is the final blow to the received view. I now close this chapter with a brief consideration of the epistemic situation that is purportedly distinctive for historical science. My aim is to undermine the distinctiveness claim.

## **1.6 Is the epistemic situation distinctive for historical sciences?**

I now turn to the question of whether so-called historical sciences constitute a distinctive category. As mentioned above, the received view holds that the distinctive challenges associated with reconstructing the past lead historical scientists to pursue an alternative inferential strategy. The focus of this section is whether the challenges and additional epistemic hurdles really are distinctive.

The most obvious aspect of so-called historical sciences is the temporal location of the target of investigation. Surely it is the fact that the subject matter of, say, paleontology, is in the deep past that makes it distinctive, for paleontologists are not in the business of predicting the outcomes of present day controlled experiments. Since the target of a historical investigation existed so long ago, it is not currently available for experimentation. In fact, the target system, for many of these investigations, no longer exists, so we cannot generate new observations or look more closely at certain aspects. And, perhaps especially problematically, one cannot

intervene on the target system to test hypotheses about how the dominant dynamics governing the system work. Moreover, it so happens that the kinds of systems that tend to be targeted by “historical sciences” are so large, or the dynamical processes so slow, that even if we could intervene on them, we are unlikely to be able to produce the kinds of interventions that would matter, nor would the effect be realized within our lifetime.

Studying phenomena that are inaccessible to experiment because they are located in the past does seem problematic and counter to what philosophers often stress as so crucial to experimental scientists’ ability to generate high quality evidence and confirm theories. And yet, a substantial portion of physics shares precisely this feature with “historical sciences.” Astrophysicists do not have the capacity to intervene on the target systems they are interested in. Strikingly, however, this area of science has yielded among the most highly confirmed theories in all of science, viz. the gravitational theories of Newton and Einstein. To be sure, experiments played important roles in the development of those theories, but astronomical observations were the ultimate testing grounds for them. Similarly, paleontologists are also able to put small, very local experiments to use, though the ultimate testing grounds of their theories are also furnished by contemporary observations of the system that they cannot intervene on. So, it seems, the inability to intervene on the system of interest is not distinctive of “historical sciences,” nor does it preclude the possibility of even a very high degree of confirmation.

Is the distinction-making problem, then, simply the fact that the target system no longer exists or is no longer at all like it was in the past? No. Astronomy and astrophysics again show that temporal location is simply not distinctive. Astrophysicists do not study galaxies, or quasars or anything else as they are now. Rather, the finitude of the speed of light and the size of the target systems studied and especially their great distance from us make it the case that astronomers aren’t observing them as they are now. Indeed, many of the target systems currently studied do not exist today; what astronomers are observing is how the system was, some time in the past, often the past so deep as to make paleontology look like it is studying what happened on Earth yesterday.

Perhaps the problem, then, is that, although astronomers don't see their target system as it is now, they do see it as it was. Whereas paleontologists don't see their target system as it was either; they only observe the few bits and pieces of the system that have survived the ravages of time. Here again, however, astronomy is simply not different. Astronomers don't "see the target as it was" for the light they observe is changed both by the passage of time due to the accelerated expansion of the Cosmos. It is also changed by what stands between us and the source. What they observe is scattered and polarized by dust in the interstellar medium, it is lensed in often very complicated ways by intervening gravitational potentials, and so forth. And what is required to filter out the effects of dust, lensing, and so forth, is General Relativity (to handle lensing), which, again, is based largely on astronomical observations, as well as electrodynamics (for red-shift) and theories of different forms of radiative transfer and the theories that underlie the various forms of spectroscopy used.<sup>10</sup> For example, contemporary observations of the cosmic microwave background radiation that are so often portrayed in popular as well as scientific publications are misleading, in the sense that what is shown to us is not direct observation but the product of a tremendous amount of theoretically motivated correction, filtering and processing. The sense that we have of astronomers or astrophysicists directly "seeing target systems as they were" when the light left them so long ago is based on not appreciating how much theoretical work goes into producing these beautiful images, let alone in interpreting them. The ability of astronomers to draw strong conclusions about their target systems is not based on a different method, but on the high degree of confidence we have in the underlying theoretical framework. What plays a similar role to this theoretical framework in paleontology is the theory of evolution coupled with a theory of fossilization from taphonomy. Moreover, sometimes the production of a "true image" of a target object is secondary, and what is revealed by how light interacts with intervening systems is more significant. Sometimes, that is, target objects can be used as probes of the intervening dynamical processes and structures.

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<sup>10</sup>Note too that this issue of the light being altered by what intervening goings-on is, to be sure, at times annoying noise that is a hindrance and must be filtered out. But it can also be incredibly revealing. The in-transit alterations of light from distant sources is often the only source of information about, e.g., chemical compositions and structure, of a wide variety of astrophysical systems.

Cleland (2002) claims that the fact that “historical sciences” study things in the past forces practitioners to employ Sherlock Holmes style reasoning; Holmes reasons from leftover effects to the causes that produced them. The idea here is that paleontologists, say, don’t have access to the phenomena but are forced to make do with bits that are left over from that time. But this claim is problematic, for it is either false or simply fails to be distinctive of sciences investigating the past.<sup>11</sup> As discussed above, astronomy and astrophysics are in similar shape. But what about other areas? If Cleland’s claim is right, then it must be the case that our best, most rigorously confirmed theories must not be in this position. It must be the case, that is, that experiments in particle physics at the LHC, say, must not rely on trace-based reasoning. This is far from the case, however. While it is true that they can repeat experiments, and have a great deal of control over the timing and energy scale of their collision experiments, they are in much the same boat, having to reason on the basis of the scattered remnants of collisions; particle physicists, too, reason from distal effects to the causes that produced them.

Other authors, such as Tucker (2004); Turner (2007) think that what is distinctive of “historical sciences” is the fact that studying past goings-on is necessarily the study of particulars, not regularities. If this is true, then we should find that so-called historical scientists do not seek to characterize law-like relations, focusing instead on particular matters of fact. But this is not so. Geophysicists, for example, study the Earth’s history because history provides access to dynamical processes that cannot be observed on human scales, as well as a testing ground for putative dynamical knowledge (see also Jeffares (2010)). Studying particulars, moreover, requires knowledge of law-like relations insofar as one needs a background expectation in order to find observations meaningful. In this sense, the study of particulars is an application of lawlike relations and so tests them. The theories of planetary core formation and of planetary accretion are based, significantly, on the study of a few particulars, some meteorites the

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<sup>11</sup>I leave open, here, the possibility that this way of characterizing scientific reasoning—as proceeding from traces or effects to underlying causes—is false. The reason for hedging slightly is simply that some may simply reject this characterization, as does Duhem (1991). If the reader rejects the idea that science can ever uncover underlying causes, then surely this will be applied to both experimental as well as “historical sciences.” Moreover, I generally eschew talking in terms of causation. I only do so here because I am addressing the received view, which is framed in this way.

Earth and Mars, and yet these theories are thought to be quite general.

Cosmology, moreover, is a prime counter-example to the idea that investigating the past is different in this regard. In fact, the standard model of cosmology involves an abstract characterization of the spacetime structure, an account of which law-like relations are dominant during which phases, and so forth. Although different authors disagree about the aim and scope of cosmology, they all seem to agree that, despite the uniqueness of the Universe, the various epochs of cosmological history ought to follow from generalized nomothetic relations and should depend as little as possible on implausible turns of events or peculiar, finely tuned, initial conditions. Though I have some qualms about how many cosmologists think about initial conditions, cosmology is a clear counterexample to the idea that studying the past of a unique system cannot simultaneously be the search for theories of general phenomena and laws. Indeed, the discovery that dark energy (or some other mechanism to be discovered in the future), not to mention inflation with its associated inflaton field is needed both to make sense of the past but also the future of the Universe shows that the distinguishing feature of cosmology is not that it is the study of a particular or token phenomenon, as opposed to a general one. And cosmology is an important context within which theories of quantum gravity are currently being formulated and tested. It is simply not the case that the study of the history of a unique particular cannot constitute an important test of general theories and hypotheses in physics.

There is yet another possibility to consider for the distinctive challenge that “historical scientists” face that is related to the inability to intervene on the target system. One of the immense benefits of experimentation is the ability to tell the difference between the effects of initial conditions and the effects of the relevant dynamical processes. The role of many experiments is precisely to investigate this. Indeed, we cannot understand and theorize about underlying dynamics without simultaneously theorizing about which aspects of the final state of a system are determined simply by the initial state. To do this, scientists vary the initial conditions and then watch how the system behaves or evolves. In the absence of making this crucial determination, all we have is a system that changed through time with no ability to



attribute certain aspects of the system's final state to initial conditions and certain aspects of the evolution to dynamical processes. This, it seems, is a severe limitation, and it is not one shared by astronomy and astrophysics, at least in all cases.

One way in which astrophysicists can distinguish between dynamical laws and initial conditions—viz. by considering ensembles of similar systems—is also employed by many other areas. This can lead to knowledge regarding initial conditions in two ways. Considering an ensemble of systems might, e.g., allow one to determine what range of initial conditions might be realized and so give some indication as to what might be necessary features of systems of that type. The approach is also useful when we want to know how a kind of system naturally develops, and where we have observational access to an ensemble of instances at varying stages of the natural evolution. Astrophysicists can do this with, for example, the evolution of galaxies. The idea is to identify a certain class or kind of galaxy that we want to characterize, and then survey for other galaxies of that type. Once we have a catalog of these galaxies, we then surely find that the ages of the members vary. If we then have theoretical reasons to sub-categorize them in terms of stage of evolution, then what we've essentially constructed is a sequence of the stages that constitute the lifespan of that galaxy type. Such a sequence is highly informative because it aids tremendously in determining what processes dominate at various times and how they are governed. Moreover, since we know that the prevailing conditions and dynamics of the cosmos have changed through time, we are assured that we've screened off certain kinds of sensitivity to initial conditions.

This idea was nicely described by (Gould, 2002, 103–116), who illustrates it with Charles Darwin's work on coral atolls. There was interest in the 18th and 19th centuries in the classification and origin of coral atolls in the South Pacific. In this case, one cannot—at least typically—go out and test hypotheses by doing full-scale island building. It's not that we cannot build islands, it's just that synthetic islands probably won't have the relevant similarities to natural ones, and so the experiment will be of little value. But we are in a position to study how islands naturally form by looking at existing ones that are in different stages of development.

This is precisely what Darwin did (Darwin, 1842). He had the insight that the various forms of coral islands in the South Pacific do not represent different types of island forming processes. Rather, he realized that the different forms simply marked different stages in a single sequence that constitutes the natural making of a coral atoll. So even though the process of island formation takes place over a time-span that is far too long for any human to observe, existing instances that occupy different points along the way reveal what the relevant processes are and in what order they naturally occur.

Another example of this form of reasoning occurs in the study of planet formation. Instead of seeing every class of meteorite as representative of radically different parent bodies, many classes are categorized on the basis that they derive from parent bodies that were simply at different stages of a unified planet-forming process. Note, too, that structure formation simulations contribute to cosmology by providing linkages among different stages of galaxy development, including collisions and mergers because you can trace the full "merger trees" of simulated galaxies.

This strategy of using an ensemble of similar systems to determine the sequence of events constituting the evolution of a class of systems, however, is not applicable in all contexts. If the target system is truly unique, then there is no ensemble of similar systems. Note, however, that the uniqueness of the target system is relative to our background knowledge. It may be that future observations, perhaps with new kinds of instruments or technologies, find that the target is not unique. This has recently occurred, for example, in the study of the origin of the Moon. In this case, what once was a theory of our Moon's formation by way of a giant impact between proto-Earth and another Mars-sized planetesimal, is now considered to be a process that should be widespread in planetary systems. Still, though, there was little hope that observations of this moon-forming process in other systems was forthcoming, for planets form within an optically dense accretion disk. Fortunately, nature has provided us with a nearby young star with an accretion disk oriented perpendicular to Earth, and planetary embryos have been observed, one of which, it appears, though this is a preliminary result, has a moon-forming accretion

disk surrounding it (Isella et al., 2019). What is important, though, is that when the system is unique, we have a real problem with the distinction between laws and initial conditions. This, however, is not distinctive of sciences that investigate the past. Climatology suffers this problem, as do areas of geosciences not focused on the deep past, such as seismology, and many others.

Such are the candidates for the special-making feature of historical science and I've found them all wanting. I wish to close this discussion with a candidate that is distinctive of many, but not all, historical sciences, and is shared by a broad class of scientific investigations that occur within virtually every sub-field of science but that have not, to my knowledge, been discussed as a possible category. They are *story sciences*. I call them this because they are, quite literally, in the business of telling stories. I use the term 'story' in a technical sense that I'll elaborate in Ch.2. Constructing stories strikes me as importantly different from the scientific endeavor, say, to characterize the constituents of the atom. Though, as I have argued above, these endeavors are not mutually exclusive. Moreover, a vast array of investigations are doing just this, ranging from neuro and cognitive sciences, to large portions of molecular biology, chemistry and physics, not to mention medicine, psychology, psychiatry, and so on. Story-sciences are ubiquitous, though surprisingly not well studied by philosophers *qua* story-sciences.

There are two sub-divisions of the class of story-sciences, moreover. One class tells or produces story-sketches. A story-sketch is intended to be generic, and is the result of studying an ensemble of similar systems. Darwin's work on coral atolls falls in this subdivision. Some parts of paleontology and evolutionary biology are rightly placed here as well. Darwin's theory of natural selection, in many ways, is a story-sketch. Much neuroscience is in the business of telling stories of how organisms with, say, a certain kind of nervous system process different types of stimuli and what outcomes are to be expected. Indeed, much of the work that is considered in the literature on mechanistic explanation falls under this sub-division. Some of astrophysics belongs here. How do black holes form? Why are some galaxies spiral, but others

elliptical, etc.? These are questions in astrophysics that call for a story-sketch.

The other sub-division consists of sciences that tell what I call full-blown stories. That is, they are in the business of telling stories that must countenance chancy interactions that often radically alter the outcome and that cannot be predicted from the start. These stories include as many of the details or aspects of the sub-events that comprise an event that make a difference to the outcome, and what differences they make. These sciences tell stories that are specific to a particular system or event. If I ask, “how do planets form?” I am asking for a story-sketch. But if I ask how did the Earth form, I am probably asking for a full-blown story. Moreover, what triggers one to tell or seek a story versus a story-sketch is whether the system or event in question is unique. The Earth has a very particular formation story, apparently unique from every other planet in the solar system. When we ask for its formation story, we are asking a contrastive question. We might be interested, for example, in why the Earth has so much water compared to Mars. Or we might want to know why the Earth-Moon system is so bizarre, compared to every other planet-moon system we know of. In paleontology, we are asking for a full-blown story when we ask, say, why did T.Rex have such stubby little arms? The contrast, in other words, between story-sketch and full blown story is the contrast between having a general dynamical theory vs. having a particular trajectory, and where the particular trajectory may include perturbations from outside of the model based on the general dynamical theory.

The distinct feature, therefore, is that some sciences tell stories, while others develop theories that tell us about relations among quantities or of the underlying constituents of matter or forces, etc. The temporal location of the phenomena they describe does not suffice to call for a distinct methodology. Nor is their epistemic “distance” from their target phenomena a distinctive-making feature. What is needed, then, is an account of the inferential strategy required for discovering and describing events. Only then can we ask the question whether this methodology is distinct from non-story-sciences.

## 1.7 Conclusion

The aim of this chapter has been mainly ground clearing. What I have shown is that the received view has inadequately analyzed the problems posed by sciences that investigate the past. The view assumes that so-called historical sciences comprise a distinct category in virtue of the special problems that arise from the temporal location of the target phenomena. I have shown that the temporal location of the target of investigation is not a distinctive feature. It is not the case that historical scientists reason from effects to causes whereas experimentalists reason from causes to effects. Science has long been in the business of inferring the causes that make the world what it is on the basis of effects. Nor does the methodology—the overarching inferential strategy—rely on an asymmetry of time, as the overdetermination thesis implies. If that were the case, the prospect of acquiring knowledge of past events would be in trouble.

In the last section I suggested that historical sciences are part of a much wider array of scientific investigation, viz. those that tell stories of the unfolding of events, that may be distinctive. Story sciences are not restricted to investigations of the past, nor do they face an epistemic situation that is distinctive. The potentially distinctive feature is that story-sciences attempt to specify the factors that made a difference between one outcome as opposed to others of an unfolding event, which includes sub-events that bring about changes in the dominant dynamical regime. Insofar as sciences that investigate the past are properly understood as examples of this broader class of story-sciences, and given that the received view of how these sciences work is deeply flawed, a new methodological account is required.

The class of story-sciences, moreover, has two main subdivisions. One subdivision investigates events that are oft-repeated in the world. These story-sciences generate story sketches, which tell the story of how a class of like systems evolve, provided that they remain relevantly isolated. Story-sketches are generally insensitive to initial conditions, as the outcome of the subsequent evolution does not sensitively depend on the starting condition, or all or most members of the class of like systems start from the same conditions. Investigations that belong to the other subdivision attempt to produce a full-blown story of the evolution of a target sys-

tem. These stories are more detailed and aim not to describe a repeating phenomenon but to describe a particular, unique sequence of sub-events. The discipline of astrophysics, for example, comprises investigations that are story-based and not. When astrophysicists are studying, say, the radial velocities of stars in galaxies, they are looking to discover regularities that constrain theories of how matter is distributed in galaxies and what forces govern these dynamical systems. Some investigations in astrophysics pertain to the first subdivision of story sciences. An example is the investigation of galactic formation or evolution, where the question is, “how do galaxies form?” The answer to this question tells a story of sub-events that constitute the formation of a galaxy, but the story is only a sketch insofar as it is a sequence of sub-events that are predictable and regularly occur. Astrophysicists also engage in investigations of the second sub-division, when, e.g., they ask, “how did the Milky Way form?” This is a question about a particular system. Research questions of this sort typically derive from the recognition of something unusual about the target. And such questions require a detailed story that tells what happened to make the system diverge from the natural evolution that we expect. It may also be that we ask a question about a particular system because we do not have a story-sketch for a relevant class of like systems. But what usually drives investigations into a particular event or system is the presence of anomalies—departures from what we expect. Story-sketches are very useful for the second subdivision of story-sciences, for, when we have them, we can contrast the particular case with the generic outcome to reveal aspects of the particular case that are odd and, therefore, derive from some unexpected or atypical interaction, often from an exogenous source.

Lastly, I would like to point out that, in spite of all of this bad news for the received view, I think there are important insights in Cleland and Currie’s work. Cleland’s analysis of smoking guns, for example, is a great insight. Smoking gun reasoning cannot do everything Cleland hopes for, but it can still play an important role. It can still rule out competitors, and that is crucial. Moreover, as I argued above, consistency checks don’t get us as far, epistemically, as we’d like, but they really do indicate promise. In fact, simply arriving at a single story that

is consistent with the traces thought to be relevant is sometimes astonishingly difficult and a great achievement. Indeed, such a story may ultimately be found to be a just-so story and discarded, but as I'll show in Ch.2, they can still play important roles in scientific reasoning. Moreover, I completely agree with Cleland and Currie that skepticism toward historical science *carte blanche* is unwarranted. They are right to emphasize that nature is sometimes extremely generous, preserving a bounty of traces that carry a great deal of information about the past.

I now turn to Ch. 2, The aim of this chapter is to provide an alternative understanding of the methodology of testing claims concerning the past. The methodology is illustrated with the case study of the origin of the Moon. The burden of the chapter is to show that a story concerning a unique event can play the same evidential role in an investigation of history as theories can in investigations that more clearly concern lawlike relations among types of phenomena (as opposed to unique tokens). The chapter further develops the notion of “story,” and emphasises the importance of story (and theory) dependent reasoning. Note that there is some repetition of content in this chapter, as well as some telegraphing of issues that are more fully explored in Ch.3, as it is written as a stand-alone paper currently under review with the *British Journal of Philosophy of Science* (Fox (In Review)).

## Chapter 2

# How the Earth Got Its Moon: On Justly Scientific Stories

### 2.1 Introduction

Stories and story-telling have long been central to human understanding. Stories can be powerful and productive cognitive tools for ordering complex sets of information, for mobilizing creativity, and for ordering and comprehending goings-on in terms of causal relations. Stories play a crucial role, moreover, in interpreting the meaning and importance of the past and present, and, in fact, provide the means of individuating historical events and other historical entities. This is true not only when it comes to human conceptualization, imagination and in the psychology and phenomenology of understanding, but in scientific practice as well. Indeed, stories play a crucial role in scientific investigations—in fact, many roles. The following is a sketch of a story that exemplifies the kind of story I am concerned with here and which will be the focus of §4 & 5:

$4.5682^{(+0.0002)}_{(-0.0004)}$  Ga (giga anum or billion years ago), the first solid grains began to form within an opaque disk of dust and gas surrounding our newborn sun. Within  $\approx 10$  million years, a population of planetesimals had grown, some as large as



Mars is today. By  $\approx 100$  million years later, the proto-Earth had grown to  $\approx 90\%$  of its final mass. There was no Moon, yet. Suddenly, a Mars-sized planet, Theia, smashed into proto-Earth. The massive impact vaporized substantial portions of both planets, melting the rest, lofting  $> 7.4 \times 10^{22}\text{Kg}$  of impact ejecta, mainly derived from proto-Earth, into a Keplerian orbit. The two planets quickly merged, finalizing the formation of the Earth. Over time, the impact ejecta in orbit cooled and coagulated, forming the Moon (adapted from Zahnle et al. (2007)).

This is not a series of independently known research findings arranged in a narrative form for pedagogical purposes. Nor is it a heuristic guide to further research. In addition to recounting a singular, time-ordered sequence, it plays an indispensable role in the measurements and inferences that make up our knowledge of what happened so long ago. In addition to conveying the present state of scientific knowledge concerning the origin of the Moon, this story plays a constitutive role in licensing the measurements that are here reported. It plays, that is, the same kind of epistemic role that theories play in, e.g., areas such as microphysics. It is no just-so story; it is a justly scientific story.

In §2, I situate the present work in the context of the relevant literature and define what I mean by 'story' and differentiate stories from narratives and some ways in which theories are understood. In §3, I argue that the view that historical sciences are somehow methodologically distinctive is based on an impoverished picture of evidential reasoning and testing in both sciences that investigate the past as well as in the more well-studied (by philosophers) sciences that investigate the present. §4 develops an enriched picture of scientific testing and describes two key roles that theories play in traditional experimental sciences, viz. theories individuate key quantities and license, often with the help of additional working hypotheses, measuring some quantities as proxies for measuring others. The burden of §5 is to show, by way of illustration, that the story of the origin of the Moon is justly scientific, for it plays the theoretical roles described in §4, licensing what I call story-mediated measurements that enable investigators to gain access to ever more dynamical details of the target system, how it evolved as it did

and not other ways. It is the playing of this role that constitutes a stringent empirical test of the story itself.

## 2.2 Some Context

One role of stories in scientific practice that is widely recognized is to provide a heuristic account of how certain phenomena could possibly arise given assumptions about more basic entities. Maxwell, for example, famously used the metaphors proposed by Faraday of “mutually embracing curves” and lines of force as the basis of a story that accounted for electric and magnetic phenomenon (Clerk Maxwell, 1864).<sup>1</sup> This story was an example of what might be called an “ontic” story; it is a story of the underlying causal production of electric and magnetic phenomena. Maxwell’s story of lines of force and how they propagate and the relations between quantity and intensity that it engendered played a crucial role in Maxwell’s conceptual efforts and led to what many consider to be an important standard of success, viz. the consilience or unification of disparate, (at least pre-theoretically) unrelated phenomena within a single mathematical framework.

Indeed, scientific theorizing that is motivated by such ontic, how-possible stories of underlying mechanisms has been repeatedly used throughout the history of science. Productive use of stories has been recently explicated in the philosophy of science literature. Janssen (2002) has illustrated this with numerous case studies where scientists have appealed to what he dubs “COI stories,” for common origin inference, and shown how this kind of story-based reasoning has led, repeatedly, to theoretical frameworks that unify disparate phenomena.

Kao (2019, 2015) demonstrates the use of ontic stories in cases in the history of physics, while Currie & Sterelny (2017); Sterelny (2016) show with cases from biology and history, that such stories, as important as they are, are valuable, even perhaps indispensable to the relevant theorizers, for their heuristic value. How-possible stories can serve as fertile guides to concep-

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<sup>1</sup>For a detailed historical explication, see, e.g. Wise (1979).

tualization as well as to new avenues for further empirical investigation. In the end, however, these fertile stories serve as metaphorical scaffolding and are discarded once the theoretical framework has been developed. Maxwell may have never succeeded in formulating the set of equations that constitute classical electrodynamics without having conceived of electric and magnetic phenomena in terms of the story of lines of force. But once formulated, the story can be discarded, for the story goes beyond the theoretical framework that it motivated, saying how the world is beneath the nomological relations and quantities that the theory encodes. The story, in fact, is surely false, and this is fine insofar as it served as a conceptual crutch, so to speak, until such time as an adequate framework came into view. The evidence, then, for Maxwellian electrodynamics was independent of that story, and so the evidence did not bear on it; it was conceptually crucial but epistemically idle.

Stories can be part of scientific investigations in a different way; the aim of a great many scientific investigations is a how-actual account of some target phenomenon. For the remainder, this is what I mean by ‘story,’ except where noted. A large class of these investigations concern long-past goings-on. These so-called “historical sciences” are often in the business of determining how some system came to be or how a target system evolved over time from some initial condition. Endeavours to know about the past encompass a wide swath of scientific investigations in nearly every scientific domain. In historical sciences, however, the story is not scaffolding that is ultimately dispensed with once an appropriate theory has been articulated. This is because the story is more than a how-possible heuristic guide. Stories, that is, can be the output of an investigation as well as play a constitutive role in scientific inference, or so this paper will soon argue. That a scientific investigation can be in the business of discovering a sequence of events is well known in the literature. See, for example, Beatty & Desjardins (2009); Beatty & Carrera (2011); Rosales (2017). Roth (2017a) calls the product of such investigations “essentially narrative explanations.” The present work differs from these works insofar as my focus is not on explanatory virtues of narratives but, rather, on the constitutive role that stories can play in scientific measurement.

I use the term ‘story’ to distinguish it from ‘narrative’ as the latter tends to signal a rich ontology, including indispensable roles for things like plot, character, intentions, and so on. Moreover, the focus of discussions of narratives often concern the cognitive effects that they have on the audience (e.g., see the insightful recent works of Morgan (2017); Roth (2017a); Beatty (2017)). My conception of story is deflationary. A story is to be nothing more than a representation of a time-ordered sequence of states of the target system, along with an account of why the system came to occupy the intermediate states that it did and not others, as well as why the dominant dynamical regime changed, whenever it did. This is nicely captured using the phase spaces, where **A story is the representation of the trajectory of a system through a many-dimensional phase space** (see, e.g., Currie & Sterelny (2017) for a complementary view). Beatty (2017) offers a compelling discussion of the importance of narratives in understanding possibility and chance, and with a less deflationary conception of stories in terms of Belnapian branching space-times. Beatty & Desjardins (2009) examines the importance of “turning points” in historical narratives and shows that turning points are also crucial in historical-scientific accounts. Turning points, on their conception, are spacetime branching points—events that foreclose on one set of possible outcomes and open up a new branch of possibility. My conception of turning points differs, though I adopt the term because I think it captures what is essential to the narrative form. In my conception of stories, **turning points are discontinuities in the trajectory of a system through phase space, often arising from changes in the dominant dynamics, as new dynamical degrees of freedom, so to speak, become relevant.**<sup>2</sup>

Insofar as both ‘model’ and ‘theory’ are notoriously vague terms, the relations between these and stories is murky.<sup>3</sup> ‘Theory’ is sometimes used loosely to refer, say, to a collection of claims that are part of our best scientific knowledge at the time. In other contexts, the term is used to denote something like a collection of nomological relations pertaining to a single

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<sup>2</sup>There are subtle differences between these two notions of turning point, but they are not crucial to what follows. The differences are mainly related to the notions of contingency, reversibility and to questions concerning the ontology of the branching spacetime picture.

<sup>3</sup>One might also consider the relation between these and simulations.

dynamical regime under the assumption that the target system is appropriately isolated.<sup>4</sup>

Stories can be theories in the former, but not in the latter sense. This is because a story is typically based on the conjunction of multiple theories, for stories necessarily involve turning points, i.e. changes in dominant dynamics. Furthermore, the target goings-on of stories are generally not classes of phenomena; stories are often of singular events. Stories are similar to, or perhaps are, a subclass of models. I do not identify them as models, though, for ‘model’ is used in many different ways in the philosophic and scientific literature, such that I fear it is ambiguous. One might, for example, create a model of the solar system on the basis of gravitational theory, and then use it to study how planets gravitationally interact or to infer their locations at other times. I would not call such a model a story, though models like this can encode what I call the target system’s **natural trajectory**, **how the system would have evolved if it were appropriately isolated or barring changes in dynamical regime, such as a collisions or phase changes or other happenings that require appeal to different theories to characterize interactions. Natural trajectories, then, are confined to a single phase space.** Departures from natural trajectory, then, are turning points. A story is more particular, taking a narrowly characterized initial condition and then evolving the system forward through events that would not be characterizable within a single-dynamical-regime model. For example, a story of the formation of the solar system will need to countenance collisions, as well as the effects of gas, turbulence, temperature fluctuations, injections of matter from interstellar space, and so on. Stories generally include violations of the assumption of isolation as well as changes in the system’s degrees of freedom. And these, along with the initial conditions of the system, require additional assumptions that aren’t fixed by the relevant theories that are used. That necessary parameters aren’t fixed by theory is true of models, too, but stories require additional assumptions in order to piece together different dynamical theories and models.

Stories have long been regarded more skeptically than theories or models, however. Not only are the turning points of a story typically chancy, but the singular nature of the phe-

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<sup>4</sup>I’m not here arguing for a particular understanding of the term. Rather, I’m merely describing what I take to be a vague but common understanding.

nomena they are about seems problematic, insofar as they are not repeatable and so scientists cannot conduct experimental interventions on them. Hempel (1942) influentially argued that investigations of the past cannot fulfil the strictures of the deductive nomological model of explanation and, thereby, can only ever amount to offering what he called “explanation sketches.” One might put the worry thus: whereas heuristic stories are at least useful fictions, putative how-actual stories are merely just-so stories, sometimes appealing but untestable insofar as they make no empirical contact, so to speak.<sup>5</sup>

These worries have led to a rift between philosophers of science and historians and philosophers of history, which still exists today.<sup>6</sup> At the heart of Hempel’s criticism is the fact that accounts of the past, even scientific accounts, have a seemingly ineliminable narrative form, for history is not governed by law-like relations. Amongst historians and philosophers of history, the consensus seems to accept that accounts of the past are necessarily done in the narrative style.<sup>7</sup> And insofar as history is essentially narrative in form, it is, thereby, not subjectable to empirical assessment, for historical narratives are not considered to be truth-apt representations of reality as it was.<sup>8</sup> Narrative histories, then, are justified on different grounds than other types of theories. This leaves a tension with regard to historical science, as noted by (Kosso, 2009, 24), for “Narrative explanation suits the situation of historiography, but not science,” because the issues in science primarily concern testing, evidence and veracity.<sup>9</sup>

Amongst philosophers of science, the prevailing trend has been to presume that a distinctive methodology is needed in order for “historical” scientists to lay claim to producing genuine *scientific* knowledge (see, e.g., Cleland (2011); Turner (2007); Kosso (2001); Currie (2018); Chapman & Wylie (2016); Frodeman (1995); Anderl ((2018)). The worry seems to be that

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<sup>5</sup>See Currie (2019b) for a recent discussion of the problem of uniqueness of target phenomena.

<sup>6</sup>See, e.g. Roth (2017b) and Uebel (2017).

<sup>7</sup>The literature on this is extensive. See, for example, Mink et al. (1987); Roth (2017b); Kuukkanen (2015b); White (1996); Danto (1965).

<sup>8</sup>For more on this, see, e.g. Roth (1988); Kuukkanen (2015b); Kosso (2001); White (1996). Roth and Kuukkanen would perhaps say, rather, that empirical assessment is possible, just not in terms of truth-aptness, insofar as historical narratives do not represent or correspond to reality, even as it was.

<sup>9</sup>Note that I am not here arguing that historical narratives cannot be epistemically assessed. Indeed, this paper argues that, in fact, a deflated form of narrative can be so evaluated and by the same lights as other theories in science. Nor am I arguing that this deflated notion of story is sufficient for those investigating human history.

when the target phenomenon lies in the past, some kind of non-empirical confirmation is required, for the usual method of scientific testing cannot be employed. Indeed, Dawid (2013) aims to legitimize his account of non-empirical confirmation, introduced as part of an apology for string theory, by making the case that it is already employed, without controversy, in areas such as paleontology. Without a distinctive inferential methodology, that is, investigations of the past—or, in the case of string theory, of physics at energy scales inaccessible by experiment—amount to a form of just-so storytelling.<sup>10</sup>

I find this current state puzzling. Hempel's DN model of explanation has long been rejected as *the* model of explanation that all proper sciences must adhere to. Part of the reason for this consensus is, of course, the rejection of the DN model's underlying commitment to hypothetico-deductivism as the gold standard of evidence and testing (see, e.g. Salmon (1984); Woodward (1989); Kitcher (1989)). The logic of testing and confirmation is widely thought to be far more complex than HD, even if no consensus has been achieved as to a replacement for HD. In other words, I think the acceptance of historical sciences as requiring an alternative inferential method was too quick and, furthermore, that articulations of the distinctive method are overly committed to a Hempelian (or Popperian) impoverished conception of method. Even though philosophers have widely rejected Hempel's view, worry that historical scientific inferences stand in need of an alternative methodology is evidence of its lingering influence. It is still widely accepted, I contend, even if tacitly, that repetition and manipulation are not only powerful for evaluating theories or hypotheses, but that these exhaust the means of such evaluations.

Conventional wisdom among scientists and some philosophers (though perhaps tacitly) does seem to match well with aspects of Hempel's views. It is still widely believed, or so I contend, that the epistemic power of experimental sciences derives from the process of inter-

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<sup>10</sup>Not all philosophers of science reject the import of narratives. To be sure, there are philosophers of science and historians who are concerned with the role of narratives in science (see, e.g., Beatty (2017); Wise (2017); Morgan (2017); Sterelny (2016); Currie & Sterelny (2017), not to mention the growing literature on narrative explanation. This literature largely focuses on what might be called the cognitive value of narratives, how narratives facilitate understanding, etc., or on how they represent contingency or on distinguishing robust from fragile narratives, or on the virtue of explanatory power and how that serves to ground epistemic assessments of narratives. My focus is complementary but different, I want to know whether narratives, or some form similar, can have epistemic and empirical value akin to theories in more traditionally conceived areas of science.

vention followed by comparison between prediction and observation. On this view, historical sciences, if they are to be genuine sciences, must employ an alternative inferential method for three reasons. First, historical scientists cannot intervene on their target system due to its temporal location. Second, historical scientists cannot derive predictions from laws because there are no laws of history. And, third, whereas experimental scientists reason from causes to effects, historical scientists must reason from distal effects, i.e. “traces,” to their past causes. Couple to these the fact that effects are lost to the ravages of time, and sciences that investigate the past seem to be at a severe epistemic disadvantage, if, that is, they are held to the same gold standard of evidence as experimental sciences.

According to the received view<sup>11</sup>, however, the above skepticism is avoided in virtue of an alternative “historical” methodology that supposedly makes it possible to achieve epistemic par with “experimental” theories, despite the greater epistemic distance from target phenomena intrinsic to investigations of the deep past.<sup>12</sup> Although the epistemic credentials of theories of the past are to be evaluated differently, wholesale skepticism towards fields like paleontology and geology, thought to be widely held, is off the table, as it is based on a mistaken assumption that the methodology is the same.<sup>13</sup> Moreover, this alternative methodology is purportedly grounded in the putative “asymmetry of overdetermination,” wherein past causes are inferred directly on the basis of traces that somehow “overdetermine” their cause. The idea is that historical scientists study causes that are overdetermined, whereas experimentalists face the well-known challenge of underdetermination of theory by data or evidence. So, despite the inability to predict, intervene and compare, historical scientists exploit a metaphysical difference between past and present to make up for this seeming epistemic limitation.

I think the sought-after distinctiveness of historical investigations is not needed for three

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<sup>11</sup>This discussion is a brief recapitulation of §1.6

<sup>12</sup>See Cleland (2001, 2002, 2011) for the seminal articulation of this view. For further development and critical engagement, see Currie (2013, 2016, 2018); Turner (2007); Forber & Griffith (2011); Tucker (2004). For recent work on the methodology of historical science coming from a non-analytic tradition, see, e.g. Frodeman (1995); Baker (1999).

<sup>13</sup>Currie (2015)’s view is more subtle. I read him as holding that what methodologically distinguishes “historical” sciences from others is that they employ a distinctive kind of pluralism, a hyper-promiscuous pluralism that he dubs “methodological omnivory.”



reasons. First, even without an alternative methodology, the purported widespread skepticism toward historical investigations is unwarranted for it misrepresents how theories are tested, as I show below. Second, assessments of the security of scientific findings shouldn't be made at the domain level. There is much about the past that we will surely never know, but this does not entail wholesale skepticism toward all investigations of the past. There are cases where scientists have not yet, and may never, go beyond loose conjecture about some past goings-on. But conjecture is not isolated to investigations of prehistory. There are areas of experimental physics in the same predicament. Underdetermination of theory by evidence leads us to be cautious and to recognize that scientific knowledge is provisional. Assessments of the status of knowledge claims in any domain must be done by looking at the specifics of a particular case. There are surely cases in which scientists have generated knowledge of ancient happenings that are as secure as some of our best theoretical knowledge in physics.<sup>14</sup>

Third, the received view is an attempt to undermine an ill-motivated skepticism by denying that the passage of time between target events in the past and the present leads to an intrinsic epistemic limit. A more fruitful question to ask at the domain level is whether there are intrinsic limits to the kinds of knowledge possible for situated observers? And there are. Knowledge in spacetime physics, for example, is intrinsically limited by the finitude of the speed of light, which limits physicists' ability to determine the global structure of spacetime. There is a standard worry that information loss—resulting from a system's approach to equilibrium that “erases” its history—precludes us from knowing the deep past. But this is just a further intrinsic limit on observers like us who cannot time travel. It does not follow from this that all propositions about the past are fictions, or have no warrant, any more than it follows, from the limitation on spacetime theories, that no claims about spacetime are warranted. Intrinsic limits within a domain do not justify wholesale skepticism about that domain.

It does not follow from information loss that all propositions about the past are fictions

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<sup>14</sup>Which would be a bigger surprise: if scientists in the future determined that T. Rex never existed, or if they determined that the Higgs is not a fundamental scalar field? Their existence and features are inferred based on distal effects, and both seem to have equally strong claim to permanence. Despite the > 65myr between us and T. Rex, its existence and many of its features have as good a claim to permanence in science as anything does.

(useful or otherwise) or somehow not truth apt, anymore than it follows from the limitation from the speed of light that spacetime theories are not truth apt. The fact is, the speed of light limits our ability to uniquely determine the global structure of spacetime on the basis of local physics, for example, but does not preclude our knowing a great deal about local spacetime structure.<sup>15</sup> Similarly, the limit imposed by the fact that much information is lost between the past and the present precludes us from knowing all of the past, but does not preclude us from knowing some of the past. Indeed, some claims about the deep past have extremely good empirical grounds for constituting knowledge. Just as it would be inappropriate to deny intrinsic limits in, e.g. spacetime physics, to avoid skepticism, so it is inappropriate to do so when considering investigations of the past.

## 2.3 Enriching the picture of scientific testing

Many of the sciences that study phenomena in the present intervene on their target systems, but not all. All sciences, moreover, reason from traces to causes, and suffer from information loss. Moreover, while it is true that scientists studying contemporary phenomena conduct experiments sometimes with the aim of testing hypotheses or theories by comparing observed outcomes with predictions derived from theory, the confirmation of a theory or hypothesis obtained from such coincidence is actually very limited. Just as it is important to not conclude that a theory or hypothesis has been falsified on the basis of false negative experimental outcomes, agreement between prediction and observation can be a false positive result. Contrary to what is implicitly believed by many scientists and philosophers alike—though seldom explicitly stated—simple consistency between prediction and observation can only establish that a hypothesis is consistent with the observed result, that the hypothesis is sufficient for an outcome.<sup>16</sup> This does not show, however, that the hypothesis is anything more than a useful fiction

<sup>15</sup>More precisely, we usually assume that the local physics extrapolates to global physics, the issue is that local observations (of our past light cone) cannot uniquely determine global structure. Some global spacetime properties are manifestly local; we can know some things about global structure, just not all of it (Manchak (2009)).

<sup>16</sup>Note that Cleland (2001) uses a similar observation to motivate her account of the distinctiveness of historical vs. experimental science, whereas I think this observation motivates treating historical science as more of the same

or calculational tool. Given this limitation, there must be more probative ways of testing hypotheses. A richer understanding of what theories are used for and what justifies credence in them is crucial. It is true that scientists studying some prehistoric phenomenon often have very limited ability to employ experiments, nor are they able to derive predictions from theories insofar as historical happenings so often include chancy external intrusions. But before conceding that this difference constitutes a distinction in inferential method, what is needed is a less impoverished picture of the role of theories and how they are justified in sciences on both sides of the putative distinction.

There is, admittedly, something plausible about the historical-experimental distinction. Experimentalists have theories from which they can deduce experimental outcomes that can then be compared with observation; they reason from cause to effect. But, events in the past cannot be experimentally probed due to their remoteness in time, complexity and scale. Events of interest in the past tend to involve spatiotemporal and energy scales that preclude attempts to duplicate them.

“Historical” scientists reason from traces to the goings-on that produced them. Vertebrate paleontologists, for example, study immensely complex dynamical systems. But they’ve never observed the systems that they study.<sup>17</sup> They observe only their effects, the bits they left behind or other marks that they’ve made such as footprints, and so on. The critters are inferred. “Historical” scientists, that is, reason from present effects to their past causes. Moreover, the material world is ambiguous; the link between effects and their causes is a matter of difficult empirical investigation and the old problem of underdetermination bites historical scientists as hard as their experimentalist colleagues ((Turner, 2005; Forber & Griffith, 2011)).<sup>18</sup> Nor is the fact that causes leave various and widespread effects that ripple out, as effects become causes of their own effects, etc., sufficient to guarantee that enough evidence exists to break this underdetermination. Information loss due to the ravages of time is not the only type of

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kind of reasoning.

<sup>17</sup>Though, crucially, they do observe systems that are relevantly similar.

<sup>18</sup>But see Cleland (2001) for an opposing view and Currie (2018) for further analysis.

loss, either. Some states of a system screen off the past states of the system by virtue of being at equilibrium (Sober, *In Press*, 1991).

The putative distinctive-making features are differences of degree, not kind. Reasoning in even prototypical “experimental” sciences goes beyond simplistic prediction-vs.-observation testing. From an investigation’s inability to perform this kind of testing, it does not follow that the investigation cannot test its claims or that it must be employing an alternative methodology. This follows only under the false presupposition that this kind of testing is all that there is. If it were, then astronomy and astrophysics would be in trouble.<sup>19</sup> This observation already suggests that the more important distinction is not between historical and experimental, but between observational and experimental investigations. And wholesale skepticism about observational investigations is clearly not warranted.

Scientific theories and hypotheses earn their keep inasmuch as, by presupposing them, we gain access to ever-finer grained details of the causal structure of the world.<sup>20</sup> Hypotheses are usually not proposed merely for the purpose of testing them. They are proposed with the hope that, if found to be consistent, they will then be used to underwrite or enable discovery of further causal structure. Call this theory-mediated measurement: presupposing a theory in order to license inferences additional causal detail.<sup>21</sup> It is widely recognized that all measurements in science are theory-mediated. In order to make a measurement of some quantity, theories or hypotheses are required in order to connect an instrument reading to the putative theoretical quantity.<sup>22</sup> Whereas theory-ladenness is often used to undermine certain forms of epistemic foundationalism, the success of theory-mediated measurements provide epistemic or confirmational boost. A theory need not already be widely believed or highly confirmed to license measurements. Theory-mediated measurements are provisionally accepted with the require-

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<sup>19</sup>For an alternative view on astronomy and astrophysics, see Anderl ((2018, 2016) who argues that these are historical sciences pursuing the alternative methodology I am here criticizing.

<sup>20</sup>Here and throughout, I use the term “causal structure of the world” broadly, to mean what features of the world make a difference to what other features, and what differences those features make.

<sup>21</sup>Additional causal detail can be in the form of new, finer grained dependencies, say, or in the form of refined precision of measurements.

<sup>22</sup>The notion of theory-mediated measurement is a twist on the well known discussions of the theory-ladenness of observation. See Hanson (1958).

ment that, by presupposing them, they open the door to either further discovery or they lead to further refinement of the theory itself.

Successful investigations typically proceed by presupposing theory in order to learn more about the world or to make ever-more precise measurements. Theory-mediated measurements can lead to new discoveries, to refinements of the very theory presupposed, as well as to rejection of the theory if the measurement technique yields results that are inconsistent with other such measures. The use of a theory in this way, as a tool for further work, constitutes a crucial form of testing that has long been central to scientific practice. Theory-mediated measurement can provide a far stronger form of evidence than prediction-vs-observation testing (see also Chang (2004); Smith & Seth ("in press"); Smith (2014); Harper (2011)).

Let me briefly sketch the concept of theory-mediated measurement. The idea starts with the observation that the measurements required for testing of theories or hypotheses must presuppose some theory; there are no, or nearly no, direct measurements, i.e. ones that do not presuppose some theory or other. Moreover, throughout the history of science, the most important measurements of phenomena that confirmed some theory, are licensed by the theory they confirm. For example, as Chang (2004) has shown, the increased precision of constant volume air thermometer measurements of temperature constituted a higher form of evidence for the gas law and for kinetic theory. And this was so not in spite of, but owing to, the fact that this measurement technique presupposes the ideal gas law.<sup>23</sup>

The important, for my purposes, insight here is that agreement between prediction and observation can only establish a theory's consistency, within some level of precision, with observations. But if this is all there is to go on, the problem of underdetermination of theory by observation looms. A well known example is the early 17th century impasse between the Tychonic and Keplerian theories of celestial motions. Comparison between prediction and accuracy was insufficient for selecting among them. And discrepancies were not very telling

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<sup>23</sup>This works for disconfirmation, as well. For example, the measurement of the infamous 43 arcsecond per century anomaly in the orbit of Mercury, presupposed Newtonian universal gravity. Without the Newtonian framework, there is no anomaly, nor techniques to measure it. I owe this point to George Smith, personal correspondence.

as it was unclear whether they were due to incorrect values of parameters, or due to theoretical shortcomings. This was not settled until Newton, who saw that theory is what individuates the quantities considered relevant and licenses the techniques used to measure them. The real test of a theory is how it performs when presupposed in order to expose further dynamical features of the world. He was charged with begging the question, for example, for applying the action-reaction principle to quantify the gravitational interactions between not only the planets and the Sun, but to interactions among the planets themselves and even their moons.<sup>24</sup> In doing so, what Newton gained was a way to determine, not just better predictions, but, presupposing the theory and passing from the course-grained celestial motions to a finer level of causal detail opened the way for further testing. Consistency is not nothing, for it at least shows promise or that a theory or hypothesis is pursuit-worthy and, in fact, can push us to revise our prior probabilities. But entrenchment of a theory or hypothesis generally requires a higher standard of evidence. I call this higher standard, following Smith (2010), “*en passant*” testing. The idea is that a greater test of a theory or hypothesis occurs when it, being presupposed, licenses investigators to make new, higher precision measurements that reveal further dynamical details—i.e. to complicate the model of the phenomena—or that license expanding the domain of applicability of the theory to additional phenomena. In passing from investigating the consistency of the theory or hypothesis itself to using it to probe new features or to gain higher precision, more stringent testing of the theory or hypothesis is achieved.

If a story concerning prehistoric goings-on can be used in an analogous way as theories in theory-mediated measurement, and tested in passing to a finer level of dynamical detail, this will go a long way toward undermining the putative methodological distinction. Can a story of a system in the past, despite information loss and inability to perform experimental manipulation, serve in this important capacity? If so, this will show that stories can be put to scientific test in much the same way as experimental theories can; that the ‘historical’ in historical science need not flag a distinctive method of testing, for it is just science done on a

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<sup>24</sup>For more on this, see Harper (2011).

target system in a different temporal location. This is the task to which I now turn.

## 2.4 Story-telling Sciences

I now turn to the story of the origin of the Moon presented in the Introduction. This story as presented is just a sketch, for, recall, I defined a story as a representation of a target system's trajectory through phase space across turning points as well as an account of why the system traced that trajectory and not some other. Though this is a deflated type of narrative, this notion of story still demands a great deal and such stories are difficult to establish. In practice, scientists pursue an understanding of what the system's natural, unperturbed, evolution should have been, with major departures accounted for. The preferred source of this information is observations, but information loss is ubiquitous and imposes an epistemic limit on us. Because the world is "messy," it is natural to expect that we will lose any ability to infer the past from presently available effects. As causal processes overlap and disorder increases—the more time that passes between target phenomenon and the present, the worse the problem of degradation of systems and their distal effects gets. We cannot know everything that happens, and even less about what happened, though, again, this does not mean that we cannot produce knowledge about the deep past that we should have high credence in. What is needed are characteristic features that are causally related to the target system or phenomenon that have been preserved somehow.<sup>25</sup> The term most commonly used in the literature for effects used to infer the past is 'traces,' though it may not be consistently used. I conceive of traces in the following way.<sup>26</sup>

**Processes tend to leave marks on the world, just as hitting a board with a hammer makes a mark.** Most marks are not durable, so are easily "overwritten" by other processes. **But some are durable enough, or otherwise preserved, such that they can survive long after they**

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<sup>25</sup>The specification of what is required, here, will depend on domain of inquiry. The following discussion of durability is far less crucial for story-investigations of present phenomena.

<sup>26</sup>Other terms used are "relics," "timecapsules," "artefacts," "fossils," and so on. None are perfect fits. See (Currie, 2018, ch.3) for a complimentary definition of the term. There is some difference between my conception of traces and Currie's, though the difference is of no importance here.

**are made. Marks preserved are traces.**

This does not mean that traces are marks that have remained unchanged, exactly as they were. Indeed, few, if any traces are marks unchanged. What is required is that any subsequent change occurs in some regular or characteristic way. This preservation, though, introduces a further degree of epistemic distance from the target. Generally, the aim of scientific investigation is the marks and the processes that made them, not the traces themselves. An example of a trace is the cosmic microwave background radiation that cosmologists have made much use of. These photons have not been causally isolated at all, they have undergone change since they were produced early in the history of the Universe. But there are only a few ways that they have changed over the intervening  $\approx 13$  billion years—they have red-shifted, with more subtle and smaller local differences due to interactions with gas and dust, traversing gravitational potential wells, and their associated temperature has dropped considerably. But since these changes are governed by well-known physical principles, the CMB is a trace that can serve as the basis for inferring the state of the Universe long ago and the processes that produced it. Despite the fact that these ancient photons are all around us, bombarding us from every direction and changing with time, they are quasi-isolated—changes are due to very specific dynamics that change them in characteristic ways.<sup>27</sup> Quasi-isolation will vary for different kinds of systems and traces and will also depend upon the coarseness of the story at a given stage of investigation.

Isotopic abundances make excellent traces because the processes that can change them are well understood. The energy scales required to alter atomic nuclei are well beyond the energy scales of terrestrial process. Similarly with isotope ratios. Earthquakes, volcanoes and even planetary collisions aren't energetic enough.<sup>28</sup> And since radioisotopes decay in accord with well-known principles, they can be used as clocks. Radioisotopic chronometers are especially valuable traces since stories crucially involve time-ordering of processes and states.

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<sup>27</sup>The CMB has another interesting feature—it appears blue-shifted in one direction and red-shifted in the opposite direction—that tells us not about the Universe or the CMB, but about us as observers—viz. that we are moving.

<sup>28</sup>Gauthier-Lafaye et al. (1996) discovered a terrestrial process that can, a naturally occurring nuclear reactor. But its effects were localized and left additional diagnostic traces.



Another example, central to the case study below, is a result of the process of core-formation. Core-formation is a natural consequence of growth, initiating when planetesimals exceed a threshold mass. Iron and siderophiles—chemical species with greater affinity for iron—migrate toward the interior. Meanwhile, silicates, the primary constituent of rock, migrate outward, along with lithophiles, chemical species with greater affinity for silicate. The result is a body with a dense core—rich in iron and siderophiles—and a mantle—a stony outer layer rich in silicates and lithophiles. Core-formation leaves a characteristic mark. Meteorites still bear this mark, even though many derive from parent bodies destroyed long ago. Meteorites are traces that have preserved marks incurred by their parent body’s growth history.

Unambiguous traces are few, however, owing to the intrinsic limits of information loss and overlapping causal processes, as well as due to our epistemic shortcomings. Some of the information needed to construct a story must be provided, therefore, by hypotheses or even conjectures. Of course, hypotheses suggested by well confirmed theories or other trusted background knowledge are preferred, but some will be conjectural. This is not surprising, for, in addition to problems of ambiguity, events in the world are susceptible to unexpected externalities and surprising coincidences. And the stranger the situation seems, the more latitude scientists have for conjectures. The Moon, for example, is weird. It’s more than an order of magnitude larger, relative to its host planet, than any other moon. It’s orbit is weird, and it’s far further from its host (for its relative size) than any other. Given the oddness of the Earth-Moon system, its origin story seems to demand a low probability coincidence, giving theorists more leeway for conjectures. Hypotheses can supply the initial or final state of the system, intermediate states, they can characterize dynamical processes or new properties that a system gains (or loses), as well as postulate intrusions from outside of the system.

My picture is that scientists use their knowledge of various dynamical processes, along with an estimate of a system’s initial state—sometimes inferred from traces, sometimes hypothesized—to determine how the system would have evolved were it isolated in the appropriate way—the system’s natural trajectory. They then compare the predicted outcome of the natural evolution

with what they can infer about the system's subsequent states on the basis of traces. If they agree, end of story. If not, the understanding of the dynamics or the system is not, thereby, disconfirmed, just as unexpected experimental outcomes and botched experiments do not refute a theory or hypothesis. Instead, the understanding of the natural dynamical processes must be enriched, or there was an external intrusion to be accounted for, or the initial condition was inadequately characterized, or the system itself was improperly individuated. The story is then enriched accordingly, and a new trajectory is proposed. It is true that experimentalists can rerun their experiment after working to prevent the source of the botched result, while investigators of the deep past cannot. But there is a similarity that is missed if we jump on this apparent difference. Experimentalists learn to identify what caused the botched result by recognizing that many sources of failure have characteristic features; they can often determine the cause of the failure based on a quick look at the data. Failures to fulfill extra-theoretical proviso— that the apparatus is in working order, that the system is appropriately isolated, that the system was properly prepared, etc.—often have characteristic or diagnostic effects. (For more on provisos and their importance, see Hempel (1988)). Just as experimentalists diagnose experimental failures on the basis of characteristic effects, so too do investigators of the past. Moreover, the kinds of happenings that drive the kinds of target systems that investigators of the past study to depart from their natural trajectory tend to be big, and so have the potential to make characteristic marks that are potentially preserved. Violations of the kinds of provisos that experimentalists try to rule out potentially generate additional traces. And though investigators of the past cannot return the target system to the relevant initial state and let it evolve forward in time again, they can theorize about what new features the target system would have gained, or what features it might have lost, in virtue of having been deflected from its natural trajectory. They can then determine how the dominant dynamical regime has changed and predict the next natural trajectory and search for ways in which the system may have departed again.

Scientists use their understanding of the dynamics and the system to predict its natural evolution and use departures as evidence for a change in the dynamics or system and then

predict a new trajectory and iterate this process.<sup>29</sup>

An obvious question, then, is how can stories, insofar as they often must include conjectures, have any claim to truth-aptness or have any claim on our credence when they are neither subjectable to experimental test nor to direct observation? Why should we believe the story of the origin of the Moon when it is predicated on the conjecture that another planet impacted and merged with the Earth? Can stories that postulate particular entities or processes that no longer exist be subjected to empirical test?

## 2.5 Story-Mediated Measurement & the Moon Forming Giant Impact

The burden of this section is to show how the pieces introduced above are used in order to construct a story and then how the initial story, by being presupposed, can license new, higher precision measurements that then refine and add new detail to the story. My aim is to illustrate how stories—even a story that features a conjectured initiating cause—can be empirically tested through their use as epistemic tools in the same way that theories can. This interplay, moreover, is analogous to the interplay between theory, hypothesis and measurement in sciences that do not appear to be story-sciences.

The Earth's isotopic ratio of  $^{182}\text{Hf}$  (Hafnium-182) to  $^{182}\text{W}$  (Tungsten-182) is widely con-

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<sup>29</sup>My use of 'prediction' here may seem odd, given that the phenomenon already occurred. Some would perhaps call this "retrodiction." I do not because I do not see there being an important difference between what is ordinarily called prediction and what I am here calling prediction. Both are determinations of the outcome of dynamical processes on the basis of our understanding of them and estimates of the system's initial condition. If this all that one means by retrodiction is that outcome occurred prior to the prediction being made, then I have no problem with the term. But there is another sense of the term that is popular among philosophers that I reject. According to this other sense of the term, retrodiction is the process of taking the current state of a system and reversing the dynamics to determine the system's prior state. The problem with it is that most dynamics are not time-reversal invariant; one cannot simply reverse, say, the signs on some quantities and then determine the system's behavior in reverse. For more on problems with retrodiction and time asymmetry, see Eckhardt (2006). Albert (2000) argues that retrodiction can be accomplished in the case of statistical mechanics, with its well-known time asymmetry, provided that we adopt an additional principle that he calls the "past hypothesis." There are potential technical problems, see, e.g., Earman (2006); Wallace (2010), but my objection is more mundane, that we simply don't reason backward in this way, even concerning the past, we reason forward in time from some proposed or conjectured initial state.

sidered by experts to be the most important piece of evidence—the most probative trace—for the story of the giant impact origin of the Moon. Hf/W is a complex trace, incorporating both aspects, described above, of how isotopes and isotope ratios can be traces.  $^{182}\text{Hf}$  is an unstable isotope of Hafnium, decaying to  $^{182}\text{W}$  with a half-life of 8.9 Myr, so their abundances can be used as a clock. And what this clock measures is the timing of core formation. Because Hf is lithophile and W is siderophile, core-formation segregates Hf to the mantle and W to the core. Just after core-formation, the mantle has high abundance of  $^{182}\text{Hf}$  and essentially no  $^{182}\text{W}$ . Then, over time, the mantle is slowly depleted of  $^{182}\text{Hf}$  as it decays, and enriched in  $^{182}\text{W}$ . So the abundance of  $^{182}\text{W}$  in the mantle is proportional to the time since core-formation. Additionally, both Hf and W are refractory, meaning that they vaporize at extremely high temperatures, a factor that will be crucial below. These features make the Hf/W system a reliable clock because the mother and daughter isotopic abundances are insensitive to terrestrial processes subsequent to core formation. They make for an excellent trace.

But there is a substantial problem. According to the prevailing view, not just of “historical” but also of “experimental” sciences, lines of evidence that have substantial impact on credence do so because they are *independent* of the other lines of evidence. Indeed, in Cleland (2011)’s influential account of “smoking guns,” it is in virtue of their independence that smoking guns have such high probative value. A smoking gun is an independent line of evidence that, when added to the list of already known independent lines of evidence, confirms one hypothetical past cause and disconfirms rival hypotheses. Hf/W, however, is widely regarded as the most important line of evidence for the giant impact, the one that generated consensus in the field, yet it fails to be knowable independent of not just other lines of evidence, but also of the story that it is evidence for.

The reason is that  $^{182}\text{Hf}$  is a short-lived radioisotope, now extinct in the solar system.<sup>30</sup> Radioactive parents go extinct after  $\approx 10$  half-lives, meaning that Hf/W is a clock that only runs for  $\approx 100$  million years since  $^{182}\text{Hf}$  was produced  $> 4.56$  Ga. This is problematic because

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<sup>30</sup>“Short-lived” is, of course, relative to the domain of inquiry. Since, in this context, we are concerned with timescales in the hundreds of millions to billions of years, a 9 myr half-life is very short.

in order to determine the time elapsed from radioactive decay, we need to know the initial abundances of parent and daughter isotopes in the Earth's mantle just after core-formation ceased. But we cannot infer this because it is extinct. To use Hf/W as a trace, therefore, requires information or assumptions that come from other traces or hypotheses.

Geochemists have found ways around this problem, but at the cost of losing independence. Ages determined on the basis of non-independent radioisotopic chronometers are called “model ages.” The idea is that short-lived clocks can be used, but they need to be calibrated. In place of knowing the initial abundances, which, so to speak, set time 0, a short-lived clock can be calibrated by coupling it to a long-lived clock, which does not have this limitation.

This might seem puzzling. Why bother with a short-lived clock at all, if using it requires that we already have an independent method of determining the age of interest? The payoff is a more sensitive, higher precision clock over the limited lifetime of the parent isotope. The sensitivity of an isotopic chronometer is inversely proportional to the half-life of the parent. The most reliable and well understood long-lived chronometer for bodies like the Earth is based on a pair of unstable isotopes of Uranium (U) that decay to isotopes of Lead (Pb).  $^{238}\text{U}$  decays to  $^{206}\text{Pb}$ , with a half-life of 4,468Myr, while  $^{235}\text{U}$  decays to  $^{207}\text{Pb}$ , with a half-life of 704Myr.<sup>31</sup> U-Pb dating is crucial to the story, for though it has the drawback that it does not yield high temporal resolution, it also dates core formation, making it a good candidate for coupling with Hf-W. Since U is lithophile, while Pb is siderophile, core-formation resets this clock by fractionating parent from daughter. If, however, the source body never differentiated, the clock dates the last time the rock crystallized.

Lead dating provides a suggestive, but inconclusive result. The technique was pioneered in the 1940's and 50's, culminating in Patterson (1956)'s discovery, using meteoritic traces that sample the inner solar system, that the planets and asteroids have the same time of formation, 4.55 Ga.<sup>32</sup> But as the technique was refined and mass-spectrometry improved, a major problem

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<sup>31</sup>In fact, there are a few techniques based on abundances of U and Pb. The technique that is relevant here is often called the Lead-Lead method, or U-Pb concordia-discordia method. See Dalrymple (1994) for an accessible overview.

<sup>32</sup>Patterson's work was widely heralded as establishing a genetic link between the planets, asteroids and mete-

surfaced. Everything in the inner solar system formed within a very short window, with the exception of two bodies, the Earth and Moon. Whereas the oldest solids formed at 4.568 Ga (Bouvier & Wadhwa, 2010), followed by larger bodies within a few million years, the Earth and Moon appear to have formed  $> 100$  Myr later, at  $\approx 4.45$  Ga (Allegre et al., 1995).

This is a striking result, for the Earth surely formed from the same pool of material and at the same time as the meteorites. Planets begin to form within the first few million years after the start of their host star, or they don't form at all, as the gas and dust that surround a new star, and from which planets form, dissipates after a few million years. Moreover, core-formation occurs very early, so cannot account for the anomalous age of the Earth. Though strange, this anomaly suggests a link between the early history of the Earth and the Moon. Over the course of the 20<sup>th</sup> century, there had been several hypotheses proposed that explained the Moon's origin, but none, when presupposed, licensed new measurements that refined the story or added further causal details. The discovery of this anomaly, however, suggested a return to an idea that Darwin (1879) had proposed, that the Moon was once part of the Earth. Whereas all prior Moon-origin stories had failed to be useful, perhaps a return to this idea would prove empirically useful. The problem that each had, however, was that they could do no epistemic work. None of them, when presupposed, led to any further discovery or measurement; none licensed further progress on fleshing out the details of Moon's origin or anything else; none opened the way to gaining access to the causal structure of the world, including its history. And just as H-D confirmation can only show that a hypothesis is consistent with observation, these proposed stories for the Moon's origin could only pass consistency tests.<sup>33</sup> They could not do what I described in the previous section that would justify credence in them, viz. they told a story about how the target system evolved, but that story could not be fleshed out or refined. My picture, again, is that what underwrites a theory or hypothesis' claim on our credence is

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orites.

<sup>33</sup>Though, of course, they did not all pass consistency tests. One example was the hypothesis that the Moon formed independently of the Earth and was subsequently gravitationally captured. This was ruled out, even before the isotopic evidence began to accumulate, on the basis that the Moon's orbit does not seem consistent with capture. It is extremely hard to contrive a capture scenario that leads to the present orbit. Consistency tests, though limited, are of some use.

our ability to put them to epistemic work, to presuppose them to learn more about the world.<sup>34</sup>

The Hf-W chronometer had such evidential import because the age determination it afforded was the result of geochemists turning a conjecture about the origin of the Moon into a *working* hypothesis. In order to couple the Hf-W systematics to the long-lived U-Pb systematics, additional assumptions are required. One of the crucial assumptions is straightforward, viz. that Hf was fractionated from W at the same time as U from Pb. Insofar as both are fractionated by core-formation, this seems unproblematic. Except that it is. One reason is that, just like Hf and W, U is refractory, whereas Pb is volatile and therefore easily lost. Volatile loss has similar effects on lead dating as core-formation. And since the Earth is so dynamic and there have apparently been other episodes of volatile loss, the U-Pb chronometer could have been reset more than once and the matching ages of the Earth and Moon may be coincidence or the result of some common cause not associated with the Moon's origin. A second reason is that the Earth's lead age is based on estimates of the total lead isotopic budget of the mantle. Since we have extremely limited access to the mantle, its lead isotopic ratio is based on samples that are thought to be representative. This is hard to check, however, as we cannot drill to the mantle to take systematic samples. An alternative, far simpler, hypothesis is that there is a hidden reservoir of lead somewhere in the mantle that, if found and accounted for in the U-Pb calculations, would resolve the anomalous age of the Earth, making it fall in line with the meteorites, asteroids and Mars (Hofmann, 2008). By itself, the U-Pb trace is ambiguous. The young age of the Earth could be spurious, or due to heating not related and subsequent to core formation.

Coupling the two chronometers requires a reason to think that the Earth's U-Pb and Hf-W clocks were reset a second time, well after initial core formation. The giant impact conjecture supplied this reason. What would happen to the Earth, if it collided with a body large enough to loft a mass, greater than the Moon's, of impact ejecta into orbit beyond the Roche limit? The

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<sup>34</sup>The giant impact hypothesis emerged as a strong contender for the origin of Moon following a conference on the Moon in 1984. It seems that the community of scientists came to see the potential of employing the conjecture to work out the details of the isotopic history of the Earth. See Stevenson (1987) for discussion following this meeting. In many ways, the research that followed has born out this promise in remarkable ways.

answer is that, given a few assumptions about the obliquity and relative velocity of the impact, the bodies would merge and a large amount of the mantle would have been vaporized, leaving the rest molten. And there would be mixing, with at least partial isotopic re-equilibration, resulting in a second episode of core-formation, which would have reset the U-Pb and Hf-W clocks. The Moon, moreover, would bear a similar mark.

The presupposition of the giant impact, then, licenses the coupling of the short-lived chronometer to the long-lived one, enabling a higher sensitivity measurement of the timing of the giant impact itself, since that would have reset both clocks.<sup>35</sup> Outfitted with the necessary additional information from the U-Pb systematics and the giant impact story, the Hf-W clock measures not the age of the Earth, but the time elapsed between the formation of the oldest solids in the solar system and the last time that the Earth's core and mantle differentiated due to the giant impact; it dates the collision between proto-Earth and Theia. This event marked the (almost) end of the Earth's formation, and the beginning of the Moon. It does so, that is, if the Hf-W age is compatible with the U-Pb age. This is crucial, for though the Hf-W chronometer is coupled to U-Pb, the coupling does not guarantee compatibility. If the two are not causally linked, there is little reason to think that the ages would be similar. Some disagreement is allowed, however, for the Hf-W clock has better temporal resolution of fractionation events in the very early solar system.

I call what this episode illustrates “story-mediated measurement,” for the conjectural story enters into the measurement used to refine it. The outcome is a high precision, story-mediated measurement of when the Moon-forming giant impact occurred. It is also an empirical test of the giant impact conjecture. The resulting measurement indicated that the impact happened earlier than indicated by U-Pb, at 30-70 Myr after the earliest solids formed (Kleine et al. (2004)). Given my assertion that this is a higher-precision measurement, the 40 Myr range in the refined giant impact timing is large. This is because the model age sensitively depends upon finer details of the impact, especially how much Theia and proto-Earth equilibrated during

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<sup>35</sup>Provided, of course, that the giant impact occurred within the lifetime of <sup>182</sup>Hf.



their merger. This is an ongoing research program, with other story-mediated measurements that probe the early history of the Earth-Theia-Moon system in astonishing detail.

## Conclusion

I opened with a discussion of the trilemma apparent in the literature that developed in response to Hempel, wherein historical investigations are either, at best, just-so stories, incapable of epistemic evaluation, or these stories are to be assessed in terms other than their correspondence with what happened for they aren't meant to be truth-apt, or the epistemic status of historical science is secured by appeal to an alternative methodology. I claimed that this trilemma, is based on an impoverished picture of both sides of the distinction between experimental and historical science. Experimental sciences go beyond H-D testing by employing the method of theory-mediated measurement, which involves the presupposition of hypotheses or theories in order to license new measurements that may refine the very theory or hypothesis presupposed or lead to additional details about the causal structure of the world. It's true that investigations of the past cannot often conform to the strictures of H-D, but neither do experimental sciences. Stories regarding the deep past can be empirically tested, and so an alternative methodology is not required. The 'Historical' in historical sciences refers merely to the fact that the target phenomena lie further in the past, not that they pursue some distinctive alternative methodology. Historical science is just science. The origin of the Moon case illustrates a method of story-mediated measurement, whereby a story can be empirically tested. Story-mediated measurement involves the presupposition of a story in order to license measurements that may refine it. Stories, therefore, can serve the same role as theories can, as epistemic tools that license measurements and discoveries that go beyond what we have "direct" observational access to. Successful use, in this sense, is an empirical test, provided that the refinements so licensed may, in fact, be incompatible with the story presupposed. That a theory or story-mediated measurement *may* refine the theory or story is crucial, distinguishing between a genuine empirically

testable scientific story from a just-so story. Fruitful consequences of theories can seldom be independently investigated. And this failure of independence constitutes a stringent, ongoing form of empirical testing. The heralded detection of the Higgs boson could scarcely have been done independently of the standard model of particle physics. Nor could the gravitational wave detections have been made independently of the theory that predicts them. This is not, therefore, a case of begging the question. Nor is it merely an example of a conjecture having fruitful heuristic value, for the consequences of the story can't be investigated independently, without presupposing the story. And though this might seem worrying, it is a feature, not a bug.

# Chapter 3

## In Search of Coherence

### 3.1 Introduction

In Ch.1, I argued that so-called historical sciences are not distinctive kinds of science in virtue of the temporal location of the target of investigation. I showed, rather, that the distinctive feature is in the recounting or reconstruction of events. Such scientific investigations culminate, when successful, in the telling of stories. But this is not a feature distinctive of investigations of the past, nor is it a feature of all scientific investigations of phenomena in the past. Instead, we must look more carefully at the target phenomenon and the questions driving the investigation. Some studies of systems in the past aim to describe law-like regularities, while others seek to characterize how more complex events—involving changing dynamical regimes—unfold. This latter type of study can further be broken down into studies of events that occur often in nature, or only once. The study of the latter, of singular events, surely pose the greatest epistemic challenges, in general. In response to the worry that stories of singular events can't be empirically tested, I developed an account, in Ch.2, of how such stories can, in fact, be tested. I argued that presupposing a story, enlisting it to do epistemic work to reveal further information about the past tests it. Although theories of unfolding events necessarily have a story or narrative character that involves hypothetical token—as opposed to types of—phenomena, I

showed that the story can play the same kind of inferential and epistemic role as theories do in traditional nomothetic sciences.

Chapters 1 & 2, together, raise a challenge that is the focus of this chapter. For the statement of the challenge, I turn back to a quotation from Cleland in Ch.1, not to further pile on Cleland, but because she has done a service in beautifully articulating a worry that I think is fairly wide-spread:

Because much is unknown about the events in the sequence, narrative explanations have a significant fictional component, involving omissions and additions. This poses a potential problem insofar as it conflicts with the traditional emphasis in natural science on evidential warrant. The problem is exacerbated by the central role of explanation in the confirmation and disconfirmation of historical hypotheses. If the primary reason for accepting a historical hypothesis is its explanatory power and it draws its explanatory power primarily from the coherence and continuity of a quasi-fictional story, then historical natural science really does seem inferior to experimental science; in the absence of empirical warrant a narrative explanation amounts to little more than a just-so story. (Cleland, 2011)

Few doubt that the narrative form can play important epistemic roles in, say, conceptualizing or understanding, that they are useful for organizing information regarding complex systems and overlapping causes and so forth. But the concern is that when it comes to matters of evidence, coherence has to play a significant role in confirmation. But this raises the problem that fictions, just-so stories, and even conspiracy theories can be coherent. So if our best explanations developed by historical scientific investigations are narrative explanations, then their degree of coherence is the only reason to have boosted credence in them. But coherence isn't up to this task, for conjectures spinning in the void, so to speak, can be coherent.

Such is the challenge this chapter addresses. The chapter title is meant to signal three things. First, the concept of "coherence" is often explicated as "hanging together," but this is not very clear. What is needed is a clearer sense of what coherence amounts to in historical

scientific stories. Perhaps a clearer understanding of the form of coherence in stories will help in the second issue the chapter title signals, namely the search for an answer to the justificatory challenge. And so this chapter is a search for a way to distinguish just-so stories from justly scientific stories and to show that coherence in the latter rightfully makes claims upon our credence. Lastly, this chapter is a journey that visits multiple, perhaps seemingly disconnected lands, so to speak, in search of treasures to be brought together at last in the end.

My plan is to first (§3.2) consider and reject a possible way out, viz. the possibility that the narrative structure of theories of the past is not essential, but instead merely a pedagogical tool or useful literary device. This section will draw upon work done by philosophers of history. In §3.3 I'll then discuss a worry concerning narratives that has been discussed extensively in the philosophy of history literature (where it's not a worry about, but a feature of, narratives). The issue is that narratives individuate events under descriptions that necessarily reference later goings-on. As such, the event that is individuated is not the same as the one that an eyewitness would individuate, so a narrative's reconstruction does not correspond with reality as it was and so narratives are not truth-apt. My argument against such a view will turn on the deflatedness of my notion of story introduced earlier and which I'll further develop.

Then, in §3.4 I turn to considering the notion of traces. In Ch.2, I indicated that I think about traces differently. In this section I will make my view more explicit. Whereas above I argued that traces are ambiguous, in this section I'll argue that 'trace' is ambiguous, and so I'll drop the term in favor of two terms, 'marks' and 'timecapsules.' Timecapsules are the material evidence that we observe today. This section is crucial to the overall aim of this chapter, for it will further sharpen the notion of coherence that stories rely upon. In particular, I'll show that the story that timecapsules are evidence for are individuated by that story; they depend on the story for their status qua evidence.

In §3.5, I undertake four tasks: First, I use the discussion of the story-dependence of evidence to articulate a clearer understanding of coherence that I'll call "coherence of evidence." Second, I'll show that evidence for traditionally conceived theories is also strongly theory de-

pendent. This suggests that the form of coherence I'm searching for is also present in other areas of scientific inquiry. Third, I'll show that, in fact, there is an account of the evidential value of coherence that is normative and that does not rely on explanatory power. Finally, I'll show that this account applies to and sheds light on coherence of evidence in stories and that this form of coherence is absent in just-so stories. Justly scientific stories, as well as theories, make lines of evidence mutually informative, whereas just-so stories and conspiracy theories are merely consistent with the lines of evidence they pick out. This account of justification for coherence also makes sense of why the unification provided by stories is evidentially powerful, yet without relying on substantive metaphysical theses concerning unification or common cause.

In §4.2, I will then extend the case study of the origin of the Moon. The case study in Ch.2 showed how a story can license using non-story-dependent measurements to calibrate a story-dependent isotopic clock. In response to this, one might claim that the justification for the story of the origin of the Moon is parasitic on the justification for the story-independent chronometric measurements. This would be a natural claim insofar as the story-independent chronometer is based strictly on law-like relations. Given what I have argued in the preceding sections, there ought to be instances when the reasoning goes in the other direction, where story-mediated reasoning is used to calibrate story-independent reasoning. The case study in this section illustrates just that. This case study is crucial for my argument, moreover, by illustrating how stories make lines of evidence mutually informative.

## **3.2 Narratives are Essential**

Narratives are surely an indispensable and powerful cognitive tool that we use and need for the ordering and communication of information. Narrative understanding is fundamental to the understanding of complex systems, to understanding history and, indeed, to understanding ourselves, both qua individuals and qua members of a larger society. Narratives have clear

epistemic import. For many philosophers of science, though, narratives do not have evidential value, and so their value is, at best, restricted to that of helpful heuristics. As discussed in Ch.'s 1&2, such a view goes back at least as far as the 1950's. For much of the subsequent time since then, narratives in philosophy of science have been widely regarded as dispensable when scientific investigations are successful. The extent to which a narrative is indispensable, the thought seems to be, the scientific investigation is evidentially deficient and requires quasi-fictional supplementation.<sup>1</sup>

To a certain extent, it is understandable that philosophers concerned with evidence are skeptical of narratives. Insofar as philosophers of science had shied away from discussions of narratives, the literature on them developed in isolation from scientific concerns. Most of the theorizing concerning the epistemic value of narratives occurs in the philosophy of literature and philosophy of history, where issues concerning human intentions and how to interpret texts from distant civilizations take center stage. So the very idea of a narrative seems to presuppose central roles for character, plot, meaning, intentions, purposes, and so forth. Indeed, to the extent that a scientific account of some phenomenon takes the form of a narrative—and where that form involves character, plot, etc.—I don't think it unreasonable to worry about the evidential status thereof. What distinguishes better from worse narratives is their overall sense of continuity and connectedness, their ability to unify different narrative strands into a coherent whole. But this preference for more unified narratives is a psychological fact about humans that does not bear much connection to matters of evidence.

I've telegraphed in Ch.2 that my sense of 'story' is a deflationist account of narrative. In part, my deflated account is meant to address the worry that narratives involve scientifically problematic notions like plot, purpose, meaning, etc. In §3.3, I'll show how my deflationist account addresses some of the worries in the previous paragraph. Indeed, that section will further

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<sup>1</sup>The reason that I say such a skeptical view toward narratives is widespread is not because it is so often explicitly stated in the philosophic or scientific literature. Rather, it's implicit in the kind of attitudes amongst more empiricist-minded scientists and philosophers that underlies, e.g., dismissiveness towards humanities disciplines. Indeed, more than a few times have technically-minded philosophers privately informed me that stories are just not the kind of thing that can have empirical import.

motivate my deflationist approach by showing how it answers further objections concerning narratives. The importance of coherence, though, will remain central in my deflated notion. My present concern is with the idea that stories are sometimes indispensable. This discussion will serve to illuminate the, as yet vague, notion of coherence.

There has been a resurgence of interest in the philosophy of science literature on narratives. Much of this literature concerns narratives qua explanations. As such, that literature tends to run orthogonal to my concerns. As I've indicated in Ch.1, I'm skeptical that explanatory virtues can deliver the kind of evidential value that is needed without presupposing some kind of metaphysical principle that connects explanatory power, or unification, with truth. This is what Cleland tries to do for common cause explanation, and which I've argued doesn't work.

More promising for my purposes is recent work on what narratives are good at doing. I mentioned in §2.2 that turning points are an important part of stories. In several articles, John Beatty, Eric Desjardins and Isabel Carrera have shown that narratives are required when the unfolding of an event involves turning points (Beatty (2017); Beatty & Carrera (2011); Beatty & Desjardins (2009); Desjardins (2015)). In order to account for or reconstruct such events, the narrative form is essential (see also (Roth (2017a))). Although my conception of turning points differs from Beatty and collaborators (I'll discuss this difference in §3.3.1), this difference is not relevant to this point. Recall that I defined a turning point in an event as occurring when the dominant dynamical regime governing the evolution of a system changes; they are discontinuities in a trajectory of a system in a phase space representation. The Earth's impact with Theia was a turning point, as was the asteroid impact at the end of the Cretaceous. From the standpoint of the prevailing conditions on Earth, both events marked discontinuities in the form of changes to the dominant dynamical regime, even if only temporarily. Another example is the introduction of a non-native species to an ecosystem. Such introductions can radically alter the delicate dynamical balance of stable ecological systems. What Beatty, Desjardins and Carrera have convincingly argued is that the narrative form is essential for representing a system's evolution across turning points. The reason narrative is essential is that no nomological



theory can serve to describe the system's evolution precisely because multiple theories will be required, often coupled with postulations of external factors like impactors or invasive species, etc.

Moreover, the sequence of turning points very often is crucial and, again, not determinable for a nomological theory that describes the dynamics of a system. Desjardins dubs this feature of systems "path dependence" (Desjardins (2011a,b)). Not only is it the case that intrusions from the outside can radically change the trajectory of a system, but the order in which these intrusions take place often radically alters the space of possible futures of the system. The narrative form is not just suited for representing chancy intrusions and the sensitive dependence of the future on their order, it is essential for representing them (Beatty (2017)).<sup>2</sup> Moreover, such sensitive dependence on chancy occurrences means that the narrative form is going to predominantly be associated with representations of singular events. If there is a problem with such cases, the fault does not lie with the narrative form.

Another important concept that has been developed in the philosophic literature on narratives concerns "Central Subjects" and "historical entities." Writing in the period before interest in narratives among philosophers of science was low, Hull (1975) considered the narrative structure of evolutionary biology. In particular, Hull was attempting a reconciliation between philosophers of history and philosophers of science, and showed the latter that inattention to narratives in biology had led to what he thought was an inadequate understanding of the concept of biological species. For Hull, understanding that a species is a "historical entity" is essential. Moreover, when the subject matter of a scientific inquiry is comprised of historical entities, narratives are indispensable.

According to Hull:

[A historical entity] is not just any entity existing in time. It is a coherent, unitary entity that either persists unchanged or develops continuously through time. At any one moment the parts of an historical entity are interrelated by a variety of

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<sup>2</sup>I will return to the notion of path-dependence in §3.3.1.

relations, among which must be spatial proximity and at least intermittent contiguity. The parts of an historical entity must also be interrelated in such a way that the entity exists continuously throughout time. But in any case, for an historical entity to remain the same entity no degree of similarity between earlier and later stages in its development is required, as long as this development is spatio-temporally continuous. (Hull, 1975, 256)

This definition of historical entities nicely illustrates what is central to the notion of story that I am advocating. One should expect that nomological theories aren't going to be sufficient for describing trajectories that involve turning points. During any stage of the trajectory, the relevant dynamical features of the system or sub-systems are going to be different. But what is crucial is that there be some continuous "thing" that persists, even though it is described differently at different times. What Hull was emphasising was that narratives have an aboutness, they're about a subject, that gives the narrative unity that is not apparent from the perspective of a nomological theory. Hull dubbed the subject of a narrative a "central subject." Central subjects can be individuals—which are historical entities—around which the narrative is woven, so to speak. Central subjects can be individual people, but the term is broader, encompassing even lineages, nations, social movements, even ideas. What is crucial, for Hull, is that central subjects are historical.

I think this notion of a central subject is crucial for two reasons. First, it sheds light on an interesting aspect of story sciences. The origin of the Moon story, for example, has a very interesting central subject, viz. the Earth-Moon-Theia system. For most of the 20<sup>th</sup> century, the investigation was at times thought to be concern mainly just the Moon (under the co-accretion or capture scenarios), or it was concerned mainly with the Earth (under the fission scenario where the Moon is a chunk of the Earth that spun off). But now with isotopic work, also combined with impact simulations, the central subject of the story is a three-body system. Moreover, the third body no longer exists, and yet it is a part of the central subject of a scientific theory. What emerges from the story is a central subject that is different from the one that was

envisioned for most of the history of work on the origin of the Moon. This aspect of story-sciences, that the thing investigations are about emerges from pursuing details of the story, is a common feature of narratives in general. The narrative form is able to represent entities that are more complex than non-narrative theories can.<sup>3</sup>

The second reason that the notion of central subjects is crucial concerns the search for a refined understanding of coherence in stories. To make this point, I want to contrast what I've said concerning the emergence of the central subject in story sciences like the origin of the Moon investigation, with something that Hull insists upon. I don't think Hull would endorse what I've said in the previous paragraph, in other words. The issue concerns Hull's insistence:

The important feature of central subjects is that from the point of view of the historical narrative associated with them, they are individuals. The identity and continuity of such individuals *can be and must be determined independently* of the events which make up the narrative ((Hull, 1975, 255) emphasis mine).

I do not know why Hull emphasized that central subjects must be known independently of the events in the narrative. Presumably he felt that failure of epistemic independence was a weakness, that independently known lines of evidence are essential. It is possible to individuate some target systems independently of a story of what happened. This is especially true of entities that enter into story-lines (discussed below). But for many of the entities that appear in the kinds of stories I'm especially interested in, our only access to them is via the story; they are knowable only insofar as they are constitutive (i.e. more than mere heuristically useful) elements in a story. Theia is such an example. Theia is, itself, a historical entity, as well a constituent of the central subject of the story, but our only access to it is by its emergence from the story of the Earth and Moon.

Turning points, too, are very often crucial sub-events in a story, but are not independently knowable. The turning point that was the impact with Theia emerges from the investigation

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<sup>3</sup>By 'emergence' I do not mean to invoke anything like metaphysical emergence. I mean, rather, only that we come to know about the central subject insofar as it is revealed to us by the story.

into the story of the early Earth. Without the story of how the Earth ought to be, isotopically, there would be no way to know about the giant impact. The story, that is, plays a crucial role in individuating the central subject as well as the turning points that it incorporates.<sup>4</sup>

What I've done in this section is to address the question whether the narrative structure is eliminable. One cannot claim that the narrative account of some sequence of turning points is merely a listing of independently known research findings. If that were the case, the narrative form would not be doing anything essential other than serving as a rhetorical device and the continuity and connection afforded by the narrative would be inessential, for the claims constituting the narrative rely, for their epistemic status, on more reliable scientific reasoning. I think Cleland holds such a view.

Indeed, in some cases, explanations of sequences of events that involve turning points and path dependence can be perfectly adequate, even if the narrative form of them is inessential. Ereshefsky & Turner (2019) give such an example of a narrative explanation of why Cecilia bought some apples:

The train approaches as Ada nears the station. If she runs, she might catch the train and make it to class on time. If not, she'll miss class. She runs and catches the train. Having boarded the train, she happens to stand next to Benson. When Ada gets in the train, the shopping bag she happens to be carrying reminds Benson that he needs groceries, so he exits at the next stop. While at the store, Benson adds some apples to his shopping basket. Cecilia, who happens by at that moment, is also shopping for fruit. Seeing Benson take some apples, Cecilia decides to go ahead and buy some.

Here, the narrative form is inessential because it really does simply present a series of independently knowable facts. These facts are causally linked, and time-ordered, and so there is continuity. What, in this case, is lacking that would make the narrative form essential is a central subject and epistemic links between the different elements of the story.

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<sup>4</sup>I will return to the issue of the role stories play in individuating entities in §3.4.

But cases like this one are not typical of those generated by scientific investigations. The narrative form is essential for a tremendous variety of cases of scientific interest, for it is rarely the case that narrative accounts solely involve independently known findings. The upshot of this work on the narrative form concerning coherence is this. The issue is not so much about conjectures or quasi-fictions freely spinning in the void. That is, the picture of coherence as “hanging together” can be refined and this refinement will lift some of the worry concerning justly scientific stories vs. just-so stories. The form of coherence involved is perhaps better understood as a form of holism. By holism I mean to stress that many things are known together, or not known at all.

I have much more to say by way of refining the notion of coherence, as well as an argument concerning the circularity worry about coherence. I will do so in §s3.4&3.5. I wish to pause the main line of development of the chapter in the next section, however, as the discussion so far has raised a few issues that need to be addressed. One of these issues concerns a view about narrative histories that has traction in philosophy of history according to which narratives do not have truth values insofar as events reconstructed by a narrative do not correspond with events as they were. Such a view threatens my way of thinking about the practice of story sciences, for I think the aim of, e.g., historical scientists is to give a veridical reconstruction of events as they happened. My argument will appeal to and further motivate my insistence upon a deflated notion of narrative. I said a little bit about this in Ch.2, but more needs to be clarified. As well, there is an alternative deflationist account of narrative, developed by Beatty, Desjardins, and Carrera that I have not yet addressed. I will briefly discuss why my account of narrative differs from theirs.

### 3.3 Distinguishing Stories From Narratives Saves the ‘Fixity’ of the past

I argued in the previous section that the narrative form of many scientific theories is essential. The main thrust of this chapter is to address worries concerning the possibility of confirmation of narratives given their reliance on coherence. This section is devoted to an orthogonal issue that has been raised in the philosophy of history that I think is problematic if it applies to scientific theories that have a narrative form. Though the topic of this section is orthogonal to the main story-line of the chapter, there will be some insights gained that will be applied to the main line of argument. Note, however, that what I see as a problem of narratives, many philosophers of history see as a feature.

What I’m worried about here concerns the intuition that the past is fixed and unchanging, whereas the future is open.<sup>5</sup> I’ll call the view I’m worried a “subjectivist” interpretation of narratives. According to this view, which has been gaining traction amongst philosophers of history for some time, narrative histories individuate events by essential reference to facts about the past that are made true after the fact. That is, the The issue here is that the set of true propositions that participants or direct observers of some past event could enumerate, even given perfect knowledge and infinite time to formulate and write them down, will not, indeed cannot, record all of the true propositions that can be enumerated later, in retrospect. Danto (1965) framed what I am referring to in terms of an “ideal chronicle.”<sup>6</sup> The idea is that some genius, along the lines, say, of a Laplacian demon who knows the totality of facts about the world at any given moment, records everything that happens, as it happens. This long list is the ideal chronicle. But, or so Danto argues, the ideal chronicle encoding, by hypothesis, all of

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<sup>5</sup>In fact, this intuition, the fixity of the past and openness of the future, is notoriously difficult to make precise, let alone consistent with the picture of spacetime we get from relativistic physics. Reconciling these issues, however interesting and needed, is not my concern in this paper. For an analysis that is perhaps most complimentary to my approach, see Ismael (2013).

<sup>6</sup>The argument that I am presenting employs Danto’s notion of the ideal chronicle, but this argument goes beyond Danto’s aims. The argument is most clearly articulated by Uebel (2017); Roth (2012, 1988); Kuukkanen (2015a,b).

the facts about the history of the world will be incomplete. There will be a host of facts about the events recorded in the chronicle that, nevertheless, will not appear in it.<sup>7</sup>

For example, suppose we have an ideal chronicle of the conflict, centered in Europe, in the second decade of the 20<sup>th</sup> century. The ideal chronicle of World War I will not include a striking truth, viz. that this event was World War I. Though true now, it was not true at the time and did not become so until much later. An ideal chronicle simply will not include truths about WWI that were made true by what happened later. This is because an event's meaning changes in light of subsequent goings-on. In virtue of what happens later, new truths seem to accrue to events in the past, so the narrative reconstruction of an event that one tells is evidently not fixed by the event itself. Narrative histories do not reconstruct events as they happened. Narratives are retrospective, including information that was not made true in the time of the event but made true later.<sup>8</sup>

Next comes the crucial move. Events are individuated under descriptions, i.e. narratives (for what I take to be deep and insightful discussions of individuation under descriptions, see Anscombe (2000); Thomson (1977)). If, in retrospect, we now individuate some past event by way of a narrative that a participant or direct observer of that event could not have assented to, then we reach a dilemma. Either we, now, do not individuate the same event that participants did, because what happens later changes what came before and so we are not talking about the same event as a participant (in this way, later goings-on change what happened and so the past

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<sup>7</sup>The ideal chronicle might satisfy Railton's notion of a complete explanatory text. The difference between Danto and Railton is that, for Danto, narrative explanations generally must include details that would not appear on the ideal chronicle, whereas for Railton, an ideal chronicle would be an ideal explanation.

<sup>8</sup>There's a deep question here as to what "facts" or "truths" are. I don't want to dwell on this issue but a few comments are in order. One important question is whether relational claims about entities existing at different times are "facts accessible to the demon"? Moreover, one might wonder whether the notion of an ideal chronicle is even coherent given the manner in which it seems to presuppose that we have a firm grasp on the distinction between present, past and future, which relativity theory calls into question. Or one might wonder whether such an ideal chronicle would be intelligible to us insofar as the Demon will understand the world very differently from us and probably "carve" the world in ways that we could not understand. Perhaps Demon describes everything in terms of positions and momenta of particles, in which case there's no way for us to recover anything resembling WWI, say. In the end, however, the intelligibility of the ideal chronicle and what facts amount to are not crucial. The argument, as I'll show next, is meant to undermine the idea that the past is determinate. The ideal chronicle, in the argument, serves to prime our intuition that the past is fixed, but the argument is meant to be a *reductio* of that intuition. And so the argument kicks the ladder out from under, so to speak, the very idea of the ideal chronicle.

is not fixed), or we can talk about the same events as past participants, but narratives fail to be truth-apt. Either way, it seems that the past is indeterminate.<sup>9</sup>

One might reject the argument above on the grounds that all that is really at issue is a quibble about language, especially naming. That a second global conflict led to changing the name of the first, seems not a very interesting change, and says more about us than it does about what happened. But I think this is too quick. Let me enrich the example a little to show why. The ideal chronicle of 1919 will leave out what I take to be a plausibly true proposition—“The Treaty of Versailles is part of the cause of World War II.”<sup>10</sup> The ideal chronicle cannot contain this proposition, for it was not true at the time. One might have worried that the reparations demanded of Germany would lead to destabilization and potential renewed conflict, but this could scarcely have been *known* at the time, even if many expected that this was a plausible outcome. Nor is this proposition, which could not be recorded by the chronicler, a trivial re-description. Since what we say now about the Treaty of Versailles is made true by what happened later, and these propositions enter into the description under-which we individuate the relevant event, we do not—indeed cannot—individuate, now, the same events that even the participants did, and so the referent of “The Treaty of Versailles” is not the same.

Moreover, participants may have held that certain goings-on were of crucial importance to the event that we, in retrospect, do not. And so, the individuation of events occurs in a context of assessments of significance. This context is especially sensitive both to later happenings, as well as to our own motivations in enquiry, both of which make it the case that the past is not fixed, that it is changed by the future. The issue is not that we come to have a different understanding of the past, it’s that the past is made different by us in the present (see Roth

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<sup>9</sup>From here, both Kuukkanen (2015a); Roth (2012) go on to argue that this also shows that the notion of the ideal chronicle above is incoherent, for there is not fact of the matter as to what events were in the past, for the past is indeterminate. Such a view is far more radical than I think is warranted by their arguments. I will not go into further detail regarding how they arrive at this more radical view, as my response will not turn on this additional aspect of their view.

<sup>10</sup>If the reader has qualms about the truth of this proposition, an alternative proposition that the reader finds more plausible may be substituted, and the following discussion adjusted accordingly. All that is needed is that the proposition be substantive and not involve the changing of the name of the event that it concerns.



(1988)).<sup>11</sup>

Such is the argument. Before responding to it, however, I want to briefly describe an alternative line of argument that converges on this conclusion. This alternative is worth considering as it does not require considering an ideal chronicle. The argument begins with the simple observation that the very same event can be described by direct participants in different ways, each in accord with what the participant saw as most salient. Coupled with the observation that events are individuated under descriptions, alternative accounts of an event clearly mean that in order for such participants to individuate the same event, we're going to need a complex description that incorporates all or most of each. In other words, we need a more complex description of the event, rather than a more basic one. But the problem is that this can't be done because direct participants' alternative descriptions may be, and often are, incompatible.

Such cases often arise in human history or accounts of conflict. One side may, for example, accuse the other side of acting with intent to do harm, while the other side sees or saw the event very differently, perhaps as one in which the other side acted with intent to do harm. Such a case is beautifully—if also disturbingly—illustrated by the film, “Rashomon” ( (Director)). The so-called Rashomon-effect is problematic insofar as it presents a dilemma. Either the conflicting descriptions aren't in conflict because they don't individuate the same event, or they do individuate the same event but there is no fact of the matter as to the true nature of the event.<sup>12</sup> Regardless of which horn one takes, there seems to be no sense in which there is an objective description under which an event can be individuated. And so there is no sense in which a past event is fixed or determinate. Evidently, observers who do not individuate the event they witnessed the same are not talking about the same event, or they are but there's no

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<sup>11</sup>Notice, then, how easy the path is, on this view, to a flavour of Holocaust denial; after all, it didn't become a “thing” until much later; it was not an “objective” event, for there are none, but merely a social construction of folks with different ideological bias. Indeed, Roth argued for such a position during Q&A at the University of Calgary mini-conference “Historical Investigations in Science,” 2015. As I indicated in the previous footnote, the view is radical.

<sup>12</sup>While it is possible that conflicting descriptions result from a deceptive observer, this is not the issue I'm considering here. Truthful and not otherwise epistemically compromised observers of an event can and often do disagree, sometimes radically, in their descriptions of events.

fact of the matter about which description is true, for past events are indeterminate.<sup>13</sup>

It’s not surprising, then, that philosophers of science in the analytic tradition, not to mention scientists, have tended to be dismissive of the epistemic import of narratives. At least since Hempel, many have already been suspicious of narratives, but the kinds of subjectivist views of narratives emerging from philosophy of history seem to raise more red flags. Throw in a common worry that a narrative history cannot be empirically tested, for they amount to just-so stories, and narratives do not look like a promising topic in epistemology of science. Or, if narratives do play a role in scientific practice, then only scientific anti-realists or social constructivists are in a position to take narratives seriously. For an anti-realist view of “historical sciences,” see Turner (2007, 2019).

The arguments for indeterminacy above strike me as implausible, for they imbue words and narratives with too much power. Surely there are facts of the matter concerning the signing of the Treaty of Versailles: there was a piece of paper, signed in a railway car, and so forth. The issue, I think, is that we can have agreement on those things without deciding how it fits into a broader narrative. The question would then be whether this was a significant event, or a “trigger” for WWII, but not whether the event actually took place. And to answer this question, subsequent events will be highly relevant. In retrospect, we typically have more information about the meaning and significance of events, even if greater temporal distance leads to loss of information about some of the details of the event.

In the discussion above of the Rashomon effect, I indicated that what seems to be needed is to individuate events by appeal to an even richer description. The problem with that, however, was that richer narratives contain more of the kind of elements that make for inconsistencies. What seems clear now is that the problem of the indeterminacy of event individuating descriptions turns on an ambiguity between, what for now I’ll loosely call, the “on the ground, causal

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<sup>13</sup>One might understand the indeterminacy in a modest way, where truth-value of narratives are indeterminate because we can’t know which narrative is the “right one.” Or, more radically, one could take the indeterminacy to be ontic, in which case there are simply no facts of the matter regarding events. As mentioned above, I’m not concerned with the issue of how radically to interpret the sense of indeterminacy here. My argument below applies to either.

order” and a richer narrative recounting of that order. Stories, in my sense, are an account of the former, whereas the problem-causing features of narratives, the latter.<sup>14</sup>

Concerning the retrospective point of view, I do not dispute that what we can say truthfully of the past can change. But the actual, on the ground causal order constituting some past event does not change in light of what happens later. And this is the sense of history being fixed that is needed. We can make the case that the past is fixed, not by going more complex but by going to a lower level of description.

When we speak of past events, we potentially have far more information than eyewitnesses would have. Though, of course, we may well have far less, owing to the intrinsic limit posed by information destroying processes that diminish and sometimes completely erase sources of information concerning the past. This extra information that we may have or may become accessible from a retrospective viewpoint can be of two kinds. Sometimes, what is learned in retrospect concerns not causal features that the event came to have subsequently. Rather, this additional information concerns the significance of the target event and what causal role it may have played in bringing about the future. So we might come to think of some event as, e.g., a particular instance of a series. Or we might come to see that some event was particularly meaningful or important as a turning point in history, say. In such cases we rightly say things about the past that no participant could have known. Yet we are not, thereby, talking about a different event. For example, one might argue that the causal structure of the world in 1919 was such that it was knowable, indeed, known, at the time that the Treaty would very likely lead to further catastrophic geopolitical instability. But since the Treaty was insufficient to guarantee this, as further elements were required to bring about WW2, it's reasonable to say that one could not be certain that it would. That it did we are now pretty certain of. But this change of certainty does not concern WW1, it concerns what happened later and the significance of WW1.

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<sup>14</sup>Note, however, that my calling them “problem-causing features of narratives” I do not mean to say that they should be eliminated. They are problematic only insofar as they are taken to be constitutive of the part of the narrative that individuates the event.

On the other hand, there might be causal structures of the world that, though perhaps unknowable at the time, we can now see very clearly in retrospect. So, for example, we might say now that, actually, the causal structure of the geopolitical world is such that the Treaty was indeed sufficient to guarantee that it was only a pause in the war. I think we often do come to learn more about the causal features of events in retrospect. In such cases, in response to discrepancies between what we say an event was in retrospect vs what an eyewitness said, I think the thing to say is that the eyewitness was wrong. Of someone, say, who thought that the Treaty was insufficient to guarantee more war, one might plausibly argue that they were simply wrong about that. But this does not entail that the event in question is indeterminate, it merely entails that we have more information available in retrospect concerning the causal structure of the world. But this deeper understanding afforded by our retrospective vantage point does not mean that our way of individuating an event is a change of subject when compared to how eyewitnesses experienced it and we'll agree on many details. This would be a substantive disagreement over the right way to describe an event, not the mark of the indeterminacy of events. Moreover, the many details on which we, in retrospect, agree with eyewitnesses, constitute the lower level of description that secures our ability to individuate the same event, even if we don't agree about higher level details.

This raises an important issue concerning the practice of history. Historical narratives do at least two distinct, though difficult to disentangle, things: they reconstruct a causal sequence, and they attempt to situate different elements of this sequence within an ordering of significance. And this ordering of significance is seldom constrained to, say, evaluating which causal factor was chanciest or played the most important role in triggering the event or some part thereof. Rather, significance is evaluated with respect to what subsequent events the sequence in question played key causal roles in shaping. I am not claiming this is necessarily problematic. Rather, the claim is simply that the peculiar feature of historical narratives, that they change in light of subsequent events, is because narratives contain both a reconstruction of the causal order of a target event, as well as assessments of significance to what comes later. A

historical narrative not only reconstructs the causal order that constituted some event, but also assesses the “meaning” of that event to, and in terms of, what comes later, and only the former elements of the narrative go into the event-individuating description. Including the latter assessments in the individuation of an event amounts to an over-description. Note, however, that I’m not claiming that the over-descriptive content is necessarily false or must be problematic in some other way. Rather, I mean only that the over-descriptive content is parochial, and, therefore, tells us as much or more about the observer than the target event. Insofar as one is interested only in the event, narrative content that pertains to the observer is, therefore, excess. (This will be made more precise below.) What leads from the observation that narrative histories contain truths about past events that were unknowable when those events occurred to the conclusion that such narratives don’t have truth-values or that the past is indeterminate is a lack of discipline, so to speak, in admitting too much into event-individuating descriptions.

The Rashomon effect leads to the conclusion that narratives are indeterminate in likewise fashion—by overdescription. The disciplined approach that I’m urging is modeled nicely, not to mention formally, by relativity theory in physics.<sup>15</sup> Just as different temporal and even psycho-social perspectives can lead to inconsistent descriptions, the finite speed of light can lead to inconsistent descriptions by observers of the same event. And yet, the relativity of motion and of simultaneity does not threaten the objectivity of events. Different observers will describe events differently depending upon their state of motion. And yet there is no worry that the actual event is somehow socially constructed or that the true event is indeterminate or that descriptions of events lack truth values.

The crucial recognition is that observers often over-describe events. By “over-describe” what I mean is that they include information that is observer relative. Taking observer descriptions, then, as objective is to take the wrong parts of a description as invariant. This is true in the theory of relativity and is true of histories. Relativistic physics provides us with a formal framework within which some parts of observer reports are discarded, as they are reports that

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<sup>15</sup>One could do this in General Relativity (GR), of course, but insofar as I am here only using physics for illustrative purposes, Special Relativity (SR) is simpler and sufficient.

are not invariant, they are parochial, for they tacitly reference the observers relative state of motion. What relativity tells us is that there is an invariant structure to events in the world that does not depend upon the observer's state of motion, viz. the spacetime or Minkowski interval. This is crucial, if also elementary, for it provides us with an example in which we have no problem dispensing with some of what an observer would report on the basis that we have a theoretical reason for identifying the parts of observation reports that are over-described. In fact, sciences have long been in the business of distilling observations down and re-translating in ways that are sometimes counter-intuitive, and this is a ubiquitous feature of sciences that are successful.

There are important lessons here, I think, for thinking about history and events.<sup>16</sup> Relativistically moving observers will give incompatible descriptions of an event. Insofar as events are individuated under descriptions, they individuate different events. But SR enables us to construct a representation of the event in terms of the invariant causal spacetime structure, the spacetime interval, that all observers must agree upon. And with this framework, we can easily inter-translate between the different observers' reports. We can then say why their reports are inconsistent, why they *seem* to individuate different events; we can demonstrate to them that they in fact observed the same event. Moreover, we do not have to insist that one or both of the observers are mistaken or speaking falsely because we have this invariant structure of the event that both descriptions are compatible with, and this is an important insight to apply to investigating history and to the worries of inconsistent narratives. Both observers report truthfully, and yet their reports are inconsistent with each other, neither did they observe the event in the way that it is reconstructed in SR. This is perhaps puzzling from the perspective of a simplistic theory of truth. SR enables us to abstract from the inconsistent descriptions to reconstruct the event in terms of the invariant causal structure, and, in so doing, we can hold incompatible descriptions as true. In scientific contexts, if not everywhere else, too, truth and truth-aptness are subtle notions. Inconsistent narratives can be held true, so long as the inconsistencies arise

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<sup>16</sup>I thank Jennan Ismael for prompting me to make this point explicit.

from over-description. The over-descriptive content of relativistic observers, of course, owes to the (perhaps implicit) assumption that the observers saw the event as it really was. That is false if meant in the sense that the quantities they observed are absolute, invariant properties of the target system rather than relational properties that depend upon the target system *and* the state of motion of the observer. Indeed, the invariant representation is not observed, it is an abstraction constructed on the basis of the dynamical formalism. Perhaps, then, what I ought to have said, concerning the fact that we can hold inconsistent observation reports simultaneously true, is that they are simultaneously true-with-a-wink. They are true for observers so situated.

What licenses distinguishing descriptive from over-descriptive content is a framework within which we can abstract from the parochial perspective of observers and construct a description of the event in terms of its invariant causal structure. And this enables us to then translate between conflicting observer descriptions. And herein lies, I think, an insight that is useful for disciplining event-individuating descriptions more generally. Instead of giving up on truth-aptness of historical accounts in the face of inconsistency, we need only require that we can translate between inconsistent stories and, in so doing, we are on the way to producing an invariant description.

To be sure, however, historical events, especially those crucially involving humans as causal factors, are not amenable to a formally rigorous framework in the way that relativistic physics is. But this need not preclude us from recognizing that historical accounts are surely prone to contain (much) over-descriptive content. In fact, we are accustomed to re-translating event-descriptions. One ubiquitous source of over-description is the ascription of intentions to agents. One way in which intentions lead to over-description is the simple fact that humans are evidently not very good at understanding themselves. Our true intentions in acting are often opaque to us. Assessments of causal significance that rely on ascribed intentions should be subject to increased suspicion.<sup>17</sup> The idea is that stories, in my deflated sense, do not incor-

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<sup>17</sup>If we are prone to mistaken characterization of our own intentions, does it mean we should be even more suspicious of our ability to accurately characterize the intentions of others? This question leads in directions not relevant to my purposes here. Though interesting, I leave it to the reader to think about this further. I will, however, suggest that, if anything about stories of the past fails to be truth-apt, it may well be claims about intentions in

porate what I'm calling over-descriptive content, or are reformulated when over-description is discovered. In human histories, then, events are to be individuated by stories that do not ascribe intentions to agents, for doing so will invariably introduce elements that depend upon perspective.<sup>18</sup> Since what one intends to do in acting and what one actually does often come apart, We usually don't know what what we're doing does, event-individuating stories ought only countenance what was actually done. Assessments of significance and judgments, say, of blameworthiness are not to be part of the story. This is not to say, though, that such judgments ought not be made. Rather, such judgments will be made on a better epistemic basis, for having an event individuated by an invariant story is what is needed to inter-translate between disagreeing sides and so puts us in a better position to judge. Notice, too, that these kinds of judgments are sensitive to what happens subsequently, and so need to be distinguished from the story of what happened.

Returning, then, to the example above concerning the Treaty of Versailles, the incompatibility between what an observer in 1919 would say and what we might now say about that event can be resolved. The assertion that the Treaty of Versailles was a cause of WW2 is not part of the story that individuates that event. Rather, it is part of the story that individuates WW2, so it is false that the observer's description is incompatible with what we might say today. And the fact that it was not called WW1 until later is easily accounted for by our ability to translate. It is surely true that the significance and meaning of an event is assessed with reference to what comes after, and so is always changing. History is constantly to be re-assessed. But this re-assessment doesn't change the causal structure of the events as they unfolded, as encapsulated in a story. These assessments of meaning, significance, blame, or what have you, are part of the narrative that we tell for our own understanding, and should not be confused with the story that individuates the event. Stories are subject to change in the light of further evidence, in which case the prior story was wrong or incomplete, but do not change merely in light of later

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acting.

<sup>18</sup> Ascriptions of intentions are very often the source of disagreement precisely because such ascriptions are parochial.



goings-on and are as free of content parochial to particular observers or sides as possible.<sup>19</sup>

Narratives, therefore, contain stories, but much else besides. And the worries concerning narratives turn on failing to distinguish the two. Cosmologists or astrophysicists are likely to balk, and rightly so, at talk of the import of narrative in their domain of inquiry, and yet they are often quite literally in the business of telling a story. So far, I've shown that what is needed is a deflated notion of narrative. In Ch.2 I introduced my deflated notion. In the coming §3.3.1 I will say more about this and situate my view with respect to an alternative deflationary account and indicate a few reasons why I depart from it issues that The remainder of this section is devoted specifically to my notion of story.

### 3.3.1 Stories, Refined

A story is a reconstruction of the evolution of a target system from some initial condition, across one or more turning points, to some final state. And, as much as can be known, a story tells how the system would have evolved, were it not for the identified turning points, as well as why these turning points occurred and not others. Turning points involve either an internal shift in the dominant dynamical processes or an exogenous intrusion that perturbs the natural evolution of the system. A story, then, can be nicely represented as a system's trajectory through phase space, where turning points are deflections from the system's natural trajectory, as discontinuities or sudden changes in the system's degrees of freedom. Reconstructing such a sequence of turning points constitutes the reconstruction of an event as a sequence of sub-events, which strikes me as importantly different from the scientific endeavor, say, to char-

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<sup>19</sup>There is another problem with the argument for the indeterminacy of narratives that I will not pursue further but wish to flag. The problem is, in a sense, the reverse of the problem underlying Cleland and Currie's underdetermination thesis. There, the problem is that they try to draw epistemic morals from metaphysical arguments. In the case discussed in this section, metaphysical morals are drawn from epistemic considerations. From the way in which we know about events—by individuating them under descriptions that have a narrative form which change depending upon one's viewpoint—an ontic conclusion—that events themselves are indeterminate—is drawn. But for this conclusion to follow, a further conflation is required, viz. events are conflated with narrative accounts of them. In other words, the argument presupposes that the individuation that narratives afford is ontic, when it's not. The individuation is epistemic; narratives call our attention to certain features of the world and place them within an ordered sequence, thereby making them known to us. And to the extent that events in the past are indeterminate, the indeterminacy is epistemic, not ontic.

acterize the constituents of the atom.

The notion of a trajectory through phase space, however, needs further clarification. In particular, a system’s trajectory across turning points will not be representable within a single phase space. This is because the notion of a phase space is theory dependent and turning points occur when the degrees of freedom of a system change. A phase space for a system is constructed on the basis of a theory that specifies the dynamical degrees of freedom that are relevant to the system’s evolution, under the proviso that no exogenous factors interfere or that no additional dynamical degrees of freedom not countenanced by that theory are relevant. In general, the phase space we construct and the trajectories through them) will only apply within a single dynamical regime. Since turning points induce new dynamics, a new phase space is generally required, and so the actual trajectory of the system is constructed piece-meal, as a trajectories within each dynamical regime are linked together. These single-regime phase space trajectories are what I call “natural trajectories.” Natural trajectories are representations of the system’s evolution under the proviso that no additional dynamics are in play. While turning points are discontinuities where we are forced to redraw, if you will, the phase space.

Turning points are crucially tied to natural trajectories. As has been widely recognized for a long time, scientific theories encode a great many counterfactuals. The ability to reason and project counterfactually is a hallmark of mature scientific areas. Without knowing what happens in in the absence of outside forces, there could be no theory of gravity, for example. The concept of inertial motion is ineliminable from Newtonian mechanics, and its analog, geodesic motion, is likewise indispensable in General Relativity theory. Every kind of scientific investigation needs an analog of inertia, of what happens in the absence of perturbative factors. Few domains can formulate their analog of inertia in such precise and simple mathematical terms, but a standard or baseline expectation is crucial. A body’s departure from uniform rectilinear motion reveals that it is subject to a net force. Likewise, a departure of a system from its natural trajectory indicates that there was some dynamical interaction not included in the expectation derived from the known or postulated dynamics governing the natural trajectory.

Moreover, what is accounted as part of the natural trajectory versus perturbing factors, and which in turn determines whether a turning point occurred, is relative to how the target system is defined. This is so because turning points are discontinuities in the system's trajectory through phase space, and the phase space itself is defined on the basis of what is taken to be the most salient or convenient or tractable characterization of the system. The goal, then, is to identify ever-more causal factors. The goal, in other words, is to discover and characterize all departures from natural trajectory, including what factors contribute to each departure and how much. The natural trajectory, then, gives us epistemic access to perturbing factors, to a finer grain of the causal structure of the world that was not apparent until larger discrepancies had been identified and resolved. To the extent that the story of the system's evolution serves in this capacity (recall the discussion of story-mediated access in ch. 2), investigators have reason to carry on. To the extent that they are able to gain story-mediated access to more and more causal details that led to the system's actual, not natural, trajectory, there is reason to think that the way in which the story individuates the system is on the right track. This includes the assumptions needed to identify the system's natural trajectory and any assumptions or conjectures regarding turning points.

Anomalies, in the form of departures from a system's expected natural trajectory, have evidential value. Anomalies tell when further dynamics are needed; they help reveal turning points. Anomalies are commonly thought to be bad, but they constitute important evidence. The reason that they are not automatically disconfirming evidence is that they, too, tend to be story-dependent; their status *qua* anomaly is relative to a background expectation provided by either a natural trajectory or a story that stitches together multiple natural trajectories plus turning points. Though, of course, we may find that the story is stricken by anomalies that defy our understanding of the relevant dynamics. Anomalies in the trajectory are the source of knowledge of perturbing factors, but the presence of intransigent anomalies is reason to think the story is wrong or our ignorance is too great at the moment. Perhaps the story mischaracterizes the system or the dynamics involved in the natural trajectories are incomplete or

turning points are misunderstood, and so forth. I will return to this issue in §3.4.

In Ch.1, I indicated that there are two sub-divisions of the class of story-sciences. One class tells or produces story-lines. A story-line is intended to be generic, representing a class of relevantly similar systems’ trajectories. **A story-line gives the broad form of the story of the evolution of a type of system. It is similar to my notion of a natural trajectory in the sense that story-lines also establish a baseline expectation of how a system will evolve. I use a different term for story-lines, however, because story-lines involve turning points whereas natural trajectories do not.** Story-lines tell us the types of turning points to expect, without necessarily specifying their exact timing or all of their features.

Story-lines are robust and this distinguishes them from stories concerning singular events or systems.<sup>20</sup> To be sure, historical trajectories are often extremely chancy and sensitive to very small changes in initial conditions, etc. But many kinds of systems do evolve through time along a fairly robust trajectory that is not very sensitive to small perturbations. Some sequences of events, it seems, are regularly instantiated in nature. Later Universe cosmologists, for example, have developed story-lines for various types of galaxy formation. The theory of planetary accretion is, likewise, a story-line. Story-lines are powerful tools for investigating nature. Insofar as they are robust, observing a system that departs from the state expected on the basis of a story-line is a clue to additional dynamical factors.

Gould (2002) dubbed the method of constructing a story-line (Gould didn’t use this term) the “method of convergence.” Gould argues that Darwin (1842) was one of the first to employ this method. The question that Darwin was faced with was the puzzling islands and coral formations in the South Pacific. There are a few different types of formation that all seemed to require different island building scenarios. What Darwin considered, though, was the possibility that these different formations didn’t represent unique historical trajectories. Rather, Darwin thought, perhaps the different formations represent different stages of the same historical trajectory. Under this supposition, then, the different forms observed need to be linked

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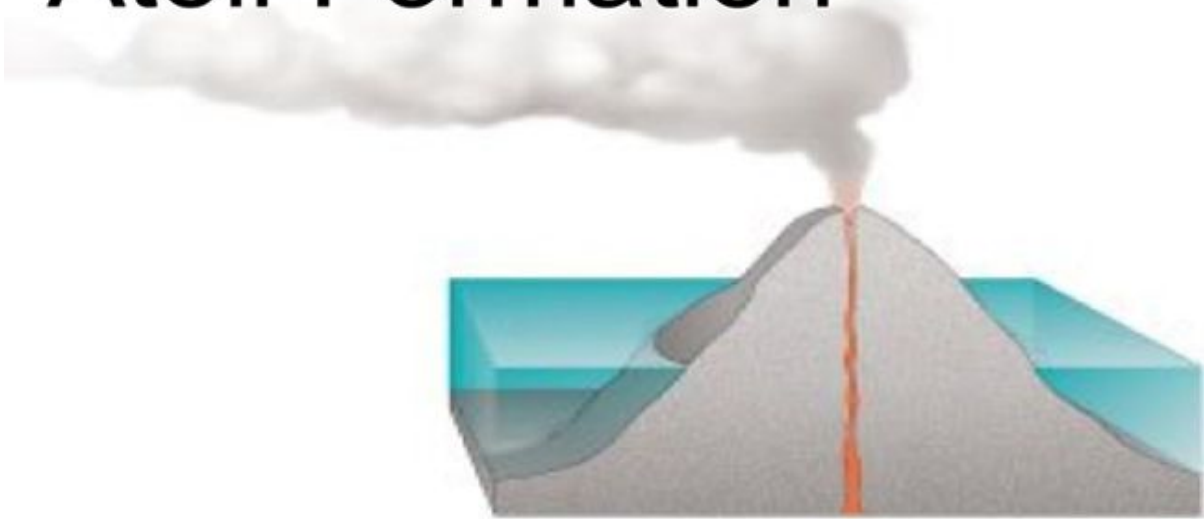
<sup>20</sup>Sterelny (2016) also emphasizes the importance of robust historical trajectories.

together by turning points. Fig.3.1 depicts the stages of the story-line. A volcanic island forms as an undersea volcano erupts and builds up breaking through the surface. Then, over time, coral begins to form, encircling the volcanic island with a fringing reef (2 in the diagram). Slowly, the volcanic island begins to sink and weather away. Meanwhile the corals continue to build up and so stay at the surface, and the now smaller island is then encircled by a barrier reef (3 in the diagram). Finally, the volcanic island further sinks and erodes to the point that it no longer breaks the surface of the water and all that remains is the coral ring, forming an atoll (4 in the diagram). In this story-line, the dynamics shift several times. Initially there is no island, then the dominant dynamics shift to volcanic activity. At some point coral growth begins, and then there is a shift again as the volcanic activity stops and weathering and subsidence dominate. Astrophysicists and late-Universe cosmologists employ this method with much success in studying the origin and evolution of galaxies and stars (see, e.g. Anderl ((2018, 2016))). So, too, is the method regularly employed in the Geosciences. The theories of core formation and planetary accretion that play crucial roles in the origin of the Moon case are story-lines developed under the supposition that various classes of meteorites represent different stages of planetary formation. The method, in essence, turns phenomena that are too large or take too long, etc. to conduct controlled laboratory manipulations on into natural experiments. The key to this kind of reasoning is observational access to an ensemble of systems that are in various stages of the trajectory.

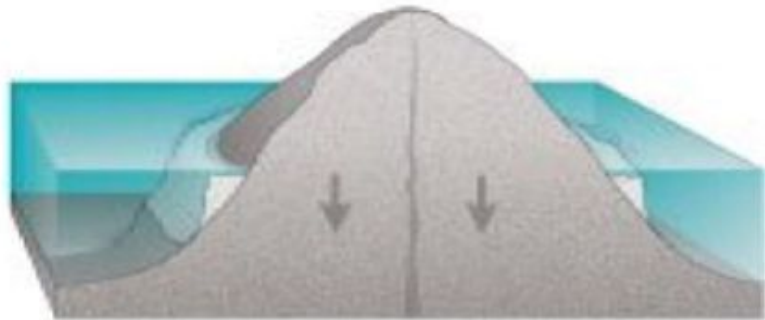
Note, too, that this kind of reasoning is involved in the crucial scientific practice of developing classification schemes. In virtue of having an ensemble of systems that share certain features in virtue of a being derived from a common story-line, scientists can consider their differences in order to determine the range of initial conditions that are commonly realized. In this way, even though story-lines are robust in that they are relatively insensitive to initial conditions, classification schemes that they license can still play a role in investigating, say, how initial conditions have varied across space and time for systems of that type.

Some parts of paleontology and evolutionary biology are rightly placed here as well, as

# Atoll Formation



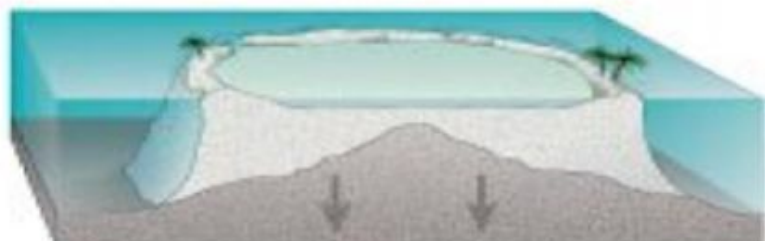
1. An underwater volcano pierces the surface of the ocean.



2. A coral reef forms around the volcanic island.



3. Fringing reef surrounds the subsiding (sinking) island.



4. Barrier reef protects a lagoon as the island completely sinks.

being in the business of constructing story-lines. Darwin's theory of natural selection, in many ways, is a story-line; natural selection specifies the form of the story of any particular lineage. Much neuroscience is in the business of telling stories of how organisms with, say, a certain kind of nervous system process different types of stimuli and what outcomes are to be expected. Indeed, much of the work that is considered in the literature on mechanistic explanation falls under this sub-division.

The other sub-division consists of sciences that tell what I call (full-blown) stories. That is, they are in the business of telling stories that must countenance departures from a natural trajectory or story-line due to turning points, which are often chancy interactions that can radically alter the outcome and that cannot be predicted from the start.<sup>21</sup> Story-lines are akin to general dynamical theories that tell what a system of that type will typically evolve if isolated. And stories are akin to a dynamical model of a particular system that includes changes to the dominant dynamical regime, at various (turning) points. Each departure, then, requires a new assessment of the system's expected evolution, and so on. These stories include as many of the details or aspects of the sub-events that comprise an event that make a difference to the outcome, and what differences they make. These sciences tell stories that are specific to a particular system or event. If I ask, "how do planets form?" I am asking for a story-line. But if I ask how did the Earth form, I am probably asking for a full-blown story. Moreover, what triggers one to tell or seek a story versus a story-line is whether the system or event in question is singular. The Earth has a very particular formation story, apparently unique from every other planet in the solar system. When we ask for its formation story, we are asking a contrastive question. We might be interested, for example, in why the Earth has so much water compared to Mars. Or we might want to know why the Earth-Moon system is so bizarre, compared to every other planet-satellite system we know of. In paleontology, we are asking for a full-blown story when we ask, say, why did T.Rex have such stubby little arms, or why did the dinosaurs go extinct so suddenly?

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<sup>21</sup>The following discussion is complementary to a view proposed by (Gould, 2002, p.104-ff), who dubs the pattern of reasoning he finds in Darwin's work on orchids the "method of discordance."

I will next turn to discussing a few ways in which my account of stories differs from an alternative deflationary account due to Beatty, Desjardins and Carrera. Before doing so, I wish to pause to summarize the this section. Previously, I indicated a few of the worries some philosophers of science and scientists have concerning the appearance of narratives in scientific theories. One of the worries concerns the possibility of testing, which I’ve addressed in Ch.2, where I argued that stories can have empirical and evidential import. I illustrated how even conjectures (or “quasi-fictions” in Cleland’s terminology) can serve as an initial story sketch and how this story can serve in the same capacity as theories do in licensing further measurements that probe deeper into the causal structure of the world. Another concerns circularity worries regarding coherence. I’ve not had my final say on this concern yet, insofar as this chapter is building towards an answer. Finally, there is the worry that narratives are, for lack of a better term, rather “squishy,” involving notions like aims and intentions, character, meaning and so forth. This section began by arguing that the narrative form is an essential part of theories in a broad range of scientific investigations. I then presented arguments that philosophers of history have made that purport to show that the truth-value of narratives is indeterminate. Moreover, these arguments turn precisely on the issues that seem worrying to empirically minded philosophers and scientists. My response was that the indeterminacy arises from taking the event individuating description to be the narrative, which contains information that is parochial to the observer. If such narratives are taken to individuate the event in question, they are overly inflated; they overdescribe events in the sense that they contain elements that are observer relative.

Instead, I argued that the problems that motivate the rejection of truth-aptness of history are overcome by being more disciplined with regards to what is taken to be the event-individuating description. A proper event-individuating description is a deflated kind of narrative that I call a story. A story is limited to information concerning the initial conditions of the system and a description of the relevant dynamics that govern its evolution, as well as a description of the various turning points or shifts in dominant dynamical regime. I have not stipulated that



human intentions or other “squishy” notions are forbidden. This is because I think that my notion of a story is important also for human history, where countenancing intentions and the like are apparently necessary. My framework is not meant to make incompatible narratives go away. Rather, my view is that we can hold incompatible narratives to be simultaneously true, in the same way that we can hold incompatible descriptions of relativistic physics to be simultaneously true. This is made possible by a dynamical theory that enables us to distinguish features of observer reports that are of quantities that are relational from those that are invariant. Now I don’t mean to suggest that the only thing that could serve in the role of dynamical theory is something formally specifiable such as a the theory of relativity in physics. When the relevant dynamical interactions involve humans, no such theory is possible, but the idea is that we do have a remarkable ability to inter-translate between different observers. So, for example, different observers will include in their descriptions of an event different assessments of significance and meaning, either depending upon their differing sides as participants in an event or depending upon their differing temporal location with respect to the event. Yet we can still inter-translate between them in order to see why their descriptions disagree. We can do so because we can, say, put ourselves in their different shoes and come to understand how they could interpret the event in that way. The dynamical theory needed, then, is something like a theory of mind.<sup>22</sup> And when we can so inter-translate, we have a new deflated story that is, in a sense, objective, and which individuates the target event under a description that does not depend on parochial features of the agents involved or observers.

So narratives presuppose or contain stories but contain additional information that is parochial. The story is what underlies the narrative in the sense that the story individuates the characters in the narrative as well as the central subject that the narrative is about. The story is to contain only information that is observer independent and concerns the causal structure of the event. The story of the Treaty of Versailles is of the form: there was a railroad car, the list of people

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<sup>22</sup>To be sure, the theory of mind is problematic in many different ways. Nevertheless, we’re still often able to understand radical differences of opinion concerning intentional actions, even if no theory of mind forthcoming could do anything like predict a natural trajectory except under extremely regimented circumstances.

in it, a piece of paper that read thus and so, signifying the end of armed hostilities, etc. The account of the Treaty will contain much else, and this is fine. Moreover, in many kinds of cases of scientific interest, the story is sufficient. It may well be that this notion of story is insufficient for human historians, but that is not my concern here. A story is meant to be a minimal deflated narrative to which more might be added depending upon the aims of inquiry.

### 3.3.2 Situating Stories

I now turn to a brief discussion that is meant to further situate my deflationist account of narratives within the philosophic literature. In particular, I want to address how it differs from an alternative view that has been articulated in a series of papers by Beatty, Desjardins and Carrera (Desjardins (2011a,b, 2015); Beatty & Carrera (2011); Beatty (2017, forthcoming)). Calling their view an alternative deflationist account, though, is perhaps misleading, for my account is largely based on theirs. The main differences arise from differences of emphasis. My aim here is to explore how my view differs from theirs and why. I will express a few worries that I have pertaining to the alternative view, but these worries are not decisive. Indeed, the differences may well be inconsequential. I’m going to raise a few concerns that I have regarding Beatty et al.’s account for the purposes of promoting discussion, but about which I do not have conclusive arguments let alone a fully worked out view.

The alternative deflated account of narratives has a similar emphasis on the turns of events that stories are essential for recounting. Indeed, I’ve adopted the term “turning point” from them. But our uses of the term differ in a way that I would like to explore. Whereas, for me, turning points are changes of dynamical regime, for Beatty et al., turning points are conceived of in terms of Belnap & Müller (2010)’s branching spacetime framework.<sup>23</sup> In this framework, turning points are spacetime branching points.

Fig.3.2 illustrates a simplified event or sequence of events. A system begins in a state denoted by A and then evolves to B. B marks a turning point, for at B something happens that

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<sup>23</sup>The branching spacetime framework is quite complex, but most of the details will not concern us here.

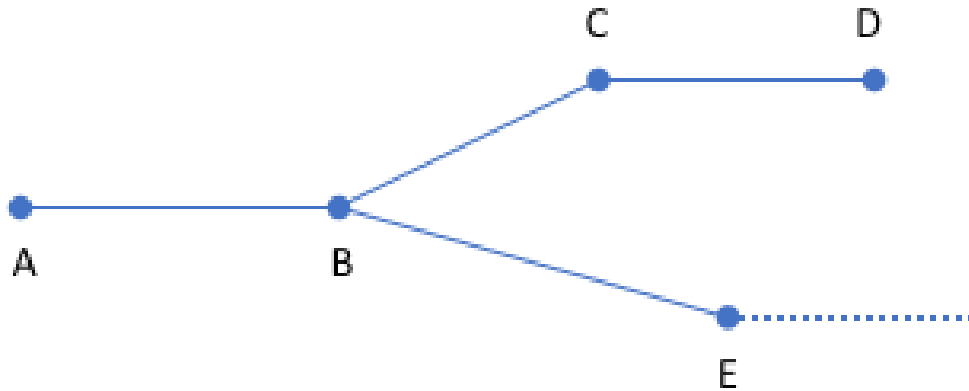


Figure 3.2: An event in branching spacetime

alters the possibility space of the system's evolution. If, at B, one chancy outcome occurs, the system will evolve from B to C and ultimately to D. Whereas if the other chancy outcome occurs, the system will evolve along the other branch, from B to E. If the system evolves from B to C, the possibility of E is forever foreclosed. Likewise, if the alternative chancy outcome at B occurs, the system will evolve to E and the possibility of the system occupying state C (and then D) is forever foreclosed. On this view, turning points are branch points, and so the only turning point in the event depicted is at B. So on this view, turning points mark foreclosures of possibility. In the example, when the system is at A, both B–C–D and B–E are possible future trajectories of the system. The main difference between this view and mine is the difference between employing this spacetime branching machinery vs. employing the machinery of phase space representations.

My main worry concerns ontology. *Prima facie*, the spacetime branching apparatus commits us to an ontology according to which spacetime branches. My own preference is to take on as few metaphysical commitments as possible and branching spacetime seems to violate this preference of mine. All else equal, I prefer a view that does not raise questions such as how to think about the ontology of spacetime branches. On my view, the event depicted in Fig.3.2 might be one where the trajectory, say, A–B–C–D is the natural trajectory, and if the

system is perturbed at B, let’s say by a giant impact, then we would have to draw a new phase space in which the system’s state will evolve to the state represented by E.

This first worry is far from decisive. For one, there is a difficult issue that my preferred phase space representation raises. The difficult question concerns how to connect the different phase spaces. In particular, the state B would need to be represented in two different phase spaces and, depending upon how different the dynamics are in the two phase spaces, this may be very challenging. It may, in fact, be that it isn’t possible and so the connection will be established by some sort of matching conditions. Indeed this is precisely what stories do, but it is difficult to see how to give a general account of how stories accomplish this. Moreover, the phase space representation isn’t meant to be a requirement, for hardly any trajectories of interest are governed by dynamics for which a phase space representation is feasible. Indeed, the phase space representation in my view is a heuristic for conceiving of and making more precise the notion of turning points. But, in the end, the phase spaces aren’t required. All that is needed is the idea that different theories are required, along with extra “nudges” from exogenous factors, to construct the evolutionary trajectory of a system. Nevertheless, this phase space conception of stories makes the nature and import of turning points clear at a more abstract and illuminating level, even if it is, in practice, incredibly challenging to write down the various phase spaces involved.

Perhaps the branching spacetime apparatus can be viewed in this way as well, as a tool for conceiving of changes in possible outcomes that turning points induce, without actually ontologically committing to spacetime having a branching structure. Unrelated to the application of branching spacetime to narrative structure, Earman (2008) proposes an interpretation that does just this, dubbed “ensemble branching.” Ensemble branching spacetime is metaphysically harmless and not so different from my own picture. Ensemble branching occurs when the dynamics of a system do not determine a unique trajectory for the system for all times past and future. Instead, the dynamics pick out an ensemble of models that are isomorphic for some finite portion of the trajectory, but then diverge. If this is the sense Beatty et al. intend

branching spacetime to be understood, then it really doesn't raise any worrying ontological issues. Understood this way, the branching picture is a way of conceptualizing the openness of the future compared to the fixity of the past.

I suspect, however, that both this interpretation of branching as well as my account of stories is insufficient for Beatty et al. I think both are insufficient for Beatty (if not also for Desjardins) because they are compatible with determinism. In the ensemble branching interpretation, insofar as the ensemble of models diverge even though they share a common past, it appears that the dynamics must be indeterministic. But the sense in which they are indeterministic is epistemic; the dynamics are not *indeterministic per se*. Epistemic indeterminism arises due to our epistemic limitations, such as when the dynamics we use involves idealizations or course-graining to make them computationally tractable. A trajectory can also be indeterministic in this sense if exogenous perturbations are allowed. Epistemic indeterminism is due to our epistemic limitations. A Laplacian demon does not have such limitations and so could determine a unique trajectory.

I suspect that Beatty has something much stronger in mind, for he does stress the importance of turning points being indeterministic *per se*. Moreover, Beatty is well known for his work on the contingency of biological evolution (see, e.g. Beatty (1984, 2006)). In that work, Beatty argues that biological evolution is contingent in the sense that it is highly sensitive to initial conditions as well as path-dependent (more on this below). But he goes on to argue that these kinds of contingency are insufficient to account for the full extent of biological evolution's contingency. For Beatty what's missing is indeterminism *per se*. Beatty argues that were we able to rewind the tape of the history of life back to the beginning and then let it play again, the resulting future forms of life would look radically different, even though the initial condition is not altered. Moreover, this heavier duty notion of contingency is what makes history matter and what makes the narrative form essential. So for Beatty et al., an important desiderata of an account of narrative structure is that it make this strong notion of contingency perspicuous in a way that ensemble branching and perhaps my own account cannot.

I think, then, that Beatty et al.’s account really does require much stronger metaphysical commitments regarding indeterminism and the metaphysics of time. The future needs to be open in a way that epistemic indeterminism does not do justice to. Epistemic indeterminism is insufficient on this view, for the future is closed. The future only seems open due to our ignorance. Belnap appears to think (and I think this is precisely what is appealing to Beatty et al.) that in order to safeguard the openness of the future, branching points must be indeterministic *per se*—not even a Laplacian demon could determine a unique future after the branching event. So, as of a branch point, it is not the case that there is exactly one branch that represents the actual future. All the possible future branches are the actual future, even though we will only ever observe one. Belnapians refer to this as the “denial of the thin red line.” Fig.3.2 indicates two possible trajectories that the system might evolve along, and prior to B we don’t know which path will be taken. One might think that there is only one path that the system will take, and that will be the actual path, and then designate by making its color red. On this stronger interpretation of the branching spacetime framework, both branches are equally actual and so there’s either no red line or both are red. This stronger interpretation of branching spacetime fits well with, and further illuminates Beatty’s “replaying life’s tape” work. The reason that each replay of life’s tape will lead to radically different outcomes is that these different historical trajectories are actual and the laws of nature do not suffice to pick one over others. So replaying the tape will lead to the exploration of alternative spacetime branches.

Relative to my aims in the present work, this stronger interpretation of the branching structure quickly runs into the weeds, so to speak. My aim in the present work is, in a sense, to make the narrative form safe again for empiricists. Insofar as the branching picture requires substantive commitments to metaphysical theses that are controversial, I would rather not make them.<sup>24</sup> Relative to my aims, then, the branching spacetime approach, while certainly a deflated

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<sup>24</sup>See, e.g. Earman (2007) for arguments that ensemble branching is the appropriate characterization of indeterminism in physics, and Earman (2008) for criticisms of this stronger interpretation of spacetime branching. See Placek & Belnap (2012) for a response. See also Belnap & Müller (2010) for a discussion of the relation between the branching spacetime framework discussed here and the branching spacetime in Everettian Quantum Mechanics.

account of narratives, is not deflated enough. My aim is only to address evidential concerns and to explicate the evidential role of stories. Beatty et al.'s account is meant to explicate other issues that are not relevant to my concerns here. In other words, the branching spacetime analysis is potentially controversial. It offers an interesting and illuminating portrayal of contingency and turning points, but given my aims, it seems sensible to remain neutral on these matters.

Indeed, much of my view could easily be construed within the branching spacetime picture if so desired. That said, there is an additional difference between my framework and Beatty et al.'s that I would like to explore concerning the nature of turning points and how this relates to the notion of path dependence. I'll then close this section by emphasizing a different aspect of path dependence that is meant to further amplify the importance of it.

One of the consequences of the strong interpretation of branching spacetime is that once a system passes a branch point, the system can never reach the states on the alternative spacetime branch. This notion of possibility foreclosure strikes me as too strong. Referring to Fig.3.2, it may be that a system at E could never get to D, provided that it remains isolated, but this does not mean that, given the right "nudge" from outside, the system cannot eventually come to state D. To be sure, sometimes this strong notion of irreversibility is appropriate. My main complaint is that this interpretation of branching spacetime makes all turning points irreversible, which might be excessive.

The issue is that once spacetime branches, there is no possibility for the system to arrive at states on the alternative spacetime branch. It seems to me that in a variety of cases of scientific interest, systems can evolve away from some state but then can come back to that state if given the right kind of nudge from the outside. Or, in other cases, systems can be perturbed and evolve away from stable configuration such as an equilibrium state, only to drift back to the stable state.

The way in which this kind of scenario is handled by the branching spacetime framework is this. The foreclosure of the possibility of state E in Fig.3.2 does not foreclose on the possibility

of a state that looks very much like E. The system might well evolve to a state that has the same configuration or is characterized by the same quantities as E. It's just that the E-like state isn't actually E, for E exists on a separate spacetime branch. Branching is such that spacetime can be branched off of, but not branched onto. In other words, different spacetime branches can share common pasts, but once a branching point is passed, branches can never converge back, their futures never cross.

This feature of the branching spacetime approach has a virtue. It beautifully represents an important feature of historical trajectories, viz. path dependence. Indeed, Desjardins (2011a,b, 2015) puts the framework to use in exploring the importance of path dependence. Nice examples of path dependence can be found in cooking. Many dishes are highly path-dependent; follow all of the steps in a recipe, but do one out of order and no amount of additional intervention can get the dish into the hoped-for, delicious, final state. Try making hollandaise sauce out of order and one will, at best, achieve an inedible mayonnaise, or worse, one might get sick. Many systems are like this. If Fig.3.2 represents making hollandaise sauce, once the system evolves from B to E, say, by misordering the steps in the recipe, no amount of additional intervention will ever suffice to get it to D. And much of history displays this kind of path dependence. Desjardins has done much to elucidate the implications of the heavily path-dependent nature of biological evolution as well as of ecological systems. If the order of introduction or arrival of either biotic or abiotic factors to an ecological system were different, the resulting system would be very different. This is why, for example, ecological restoration is so difficult, for the mere presence or reintroduction of all of the species to a damaged area will not result in restoration to the pre-damaged state. What this means is that the aim of ecological restoration should not be to recover the pre-damaged state, as this is probably not possible. Stable ecological states are not like other kinds of stable equilibria inasmuch as they are so highly contingent not just upon the presence of all of the right factors but also on their order of introduction.

This depiction of path dependence highlights a central concern of Beatty et al. which



is the sense in which history matters. On their view, history matters when the outcome is highly contingent, where the future trajectory of a system sensitively depends upon the path the system has taken so far.<sup>25</sup> Note, too, that branching spacetime also captures the notion of a historical entity that I discussed in §3.2. Recall that a historical entity the central subject of a narrative that is individuated by its history. Recognition of this kind of entity is crucial for understanding history because in a wide variety of cases, the subject of a narrative can change dramatically over the course of the narrative even to the point where there is nothing recognizably similar between the earlier form of the subject and its later form. No degree of similarity between earlier and later stages of the central subject is required. All that is required is spatio-temporally continuous development. The individuation conditions of a central subject, then, are partly constituted by its historical trajectory.

My worry, however, is that what the branching spacetime framework does well, it does too well. My concern is that all systems and states are individuated partly by their history, i.e. their spacetime branch. A constitutive feature of a system's being in state D is that it was previously at C, preceded by B, and A. Some states are like this, but not all. It's true that some recipes are highly path dependent, but many are not. Authors of cookbooks for kids tend to focus on recipes that are rather path independent. These recipes are robust in the face of many different kinds of mistakes. Other examples involve equilibrium. In the Hollandaise example, if I'm hungry enough, both the delicious hollandaise sauce at D, or the gross mayonnaise at E, will both be at the same state, F, having been digested (provided I don't get sick!). In fact, I've approached this issue earlier when discussing intrinsic limits. Recall that information loss—approaching equilibrium tends to erase a system's history—is ubiquitous.<sup>26</sup> At F, the prior history or spacetime branch of the sauces, whether A–B–E or A–B–C–D, does not matter. Branching spacetime, then, seems to commit us to the view that states of a system are partly constituted by the system's history. But since not all trajectories are path dependent, the view

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<sup>25</sup>Though, the past and present of a system are insufficient to fix the future states insofar as the the future is open, i.e. indeterministic per se.

<sup>26</sup>See also Sober (1991, In Press) for further discussion of how equilibrium screens off history.

seems to entail gratuitous branching. In the case of a robust recipe, each step is contingent, it didn’t have to be done, and so induces a branching point, but we end up generating a bunch of branches a great many of which have the same outcome.

Let me now state the point above by way of contrast with how I think about path dependence and possibility. On the branching spacetime conception, literally different possibility spaces spring into being at turning points. On my view, path-dependence is conceived of as accessibility relations among regions of phase space. Once you pass through the state corresponding to B, the trajectory diverge into two bundles, these bundles go to different regions of phase space—like rivers flowing around an obstruction—after which it is much more difficult to access other regions. Nevertheless, forked rivers sometimes re-converge.

Here there is a subtlety to note and about which I’d like to briefly sketch the way an argument might go, but which is to be the subject of future work. The issue here concerns how Beatty et al. might reply to my concern about gratuitous branching. I think what they want to say about this is that a system can return to some states, that the branching in question is really not so ontologically worrying. All that spacetime branching amounts to in regards to states of a system is that the system can never return to the past state because that state occurred in the past. The system may return to a state just like that one in every way, except that the time won’t match, or a state cannot return to past times. So on their view, the problem with my claim that once the system in Fig3.2 departs from B and comes to E, say, it can never get to C not because it cannot ever arrive at a state like C, but because C occurred at a time in the past. The issue here is that on Beatty et al.’s conception, the state of a system includes a specification of the time. And so my worry doesn’t have teeth, or them, because it involves a misunderstanding of how they define the state of a system. My reply to this (again, this is just a sketch to work out in future work) is that this way of defining states of a system is incompatible with the ordinary way in which dynamical theories compute a system’s trajectory. Indeed, it may well be computationally intractable to try to include the time into the specification of the state of a

system.<sup>27</sup> In order to so specify states, my worry is that time has to be treated very differently in the dynamics, where time is not longer an independent variable indicating succession of states in phase space but becomes, itself, a degree of freedom. I don't see this as being workable in anything like a quantitative dynamical theory. The upshot, then, of this line of reasoning is that the branching spacetime picture, if it is to be understood in an ontologically deflated way, imposes additional (significant) burdens on the dynamics of a system's evolution.

On my view, moreover, turning points need not be unpredictable. A system's evolution along a trajectory that was fully predictable from the start is likely uninteresting, or at least a story not worth telling. The key feature of turning points in my sense is that they result from a change in the dominant dynamical regime. When this happens, the dynamical description of the state changes as new dynamical properties become important. Different models will be used to capture the dominant dynamics at different spatial, temporal and energy scales.<sup>28</sup> So phase changes from solid to liquid or gas are turning points. A solid to liquid phase change involves the system's dynamical properties or capacities changing. The system loses, for example, the capacity to propagate shear waves, and gains the capacity, say, of exerting a vapor pressure; the internal atoms or molecules gain new degrees of freedom. And the characteristic length scale of the dominant dynamical interactions within the system change as well. A mixture of eggs, salt, olive oil, etc., will, if the steps are done in the right order, come to have desirable new dynamical properties, viz. the characteristic taste and texture of hollandaise sauce.

The dynamical changes associated with turning points, however, need not persist; they can have arbitrarily short duration; the dynamical properties gained or lost may only be transient. All that is required is that the system's trajectory through phase space has a discontinuity from the standpoint of the prior dynamical regime.

Contingency, though, is still important. Again, a turning point, such as a solid to liquid phase change is predictable (sort of), but it is also contingent in the sense that an interven-

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<sup>27</sup>Moreover, it may well be incoherent insofar as time is treated relativistically.

<sup>28</sup>On the spacetime branching framework, the shift induced by turning points is given a metaphysical interpretation wherein the possibility space itself changes. Whereas on my conception turning points are construed epistemically, in terms of how we represent the dominant dynamics.

tion can delay, prevent, or radically alter the nature of the dynamical change. If I modify the environment of the solid I’m heating appropriately, the liquid phase can be bypassed altogether.

So turning points on my account are of central importance, though my conception of them is different. And though I’ve said that not all trajectories are path-dependent, I still think the ubiquity of path-dependence is an important insight. Although path-dependence makes for further complexity, as path-dependent systems are highly sensitive to the order and timing of perturbations, I stress that in addition to these considerations, path-dependence can be an epistemic resource. Path-dependence makes it much harder to come up with alternative histories that might be compatible with the current state of the target system, and so can act as a constraint on underdetermination. For example, finding a sauce in a Hollandaise-like state puts severe constraints on the investigation into how that sauce formed. A more complicated example concerns the origin of the Moon case. Had the Earth’s collision with Theia occurred much earlier, Earth may very well have never become habitable. This is because Earth’s mantle, prior to the giant impact, was in its chemically reduced oxidation state. Earth is habitable, in part, because the mantle is in its oxidized state, and became so as a result of the Moon-forming giant impact. A system’s sensitivity to the timing of perturbations provides a crucial constraint on theorizing. If we have access, by way of timecapsules or, perhaps, by observations of analogous systems, to an initial and final state of a target system, path-dependence can severely constrain the possibility space of trajectories connecting those states. I will return to this discussion in the following section.

Let me now recap this section. I opened the section by arguing that the narrative form of scientific theories is essential in a broad range of cases. This discussion focused on three insights that will be important for the remainder of this work, viz. turning points, non-detachability and central subjects. As I’ll show below, these will be key ingredients in the discussion of coherence. I then considered arguments to the effect that narratives accounts of events are indeterminate. I argued that these arguments are based on an undisciplined use of narratives, where elements that are parochial to the observer are taken part of the event individuating de-

scription. What is needed, instead, is to take a deflated form of narrative, what I call a story, as the event individuating description. I then sought to further explicate my deflated formulation of stories by contrasting the manner in which I conceive of turning points and changes in possibility with Beatty et al.'s framework that is cashed out in terms of Belnapian branching spacetime. In addition to further clarifying my deflationary account of narrative, this discussion raised two more insights that will be important in the remainder of this chapter as well as in Ch.4. These two insights are the importance of path dependence and the role that stories play in individuating events and central subjects.

In the next section I will focus on the concept of traces. In Ch.2, I indicated that the way in which I conceive of traces differs from the manner in which they are typically construed in the literature. The aim of the section is to further develop my account of traces. This refined view will shed light on a crucial aspect of the scientific practice of investigating the past that has not received adequate attention in the philosophical literature. This refined view will also illuminate the crucial role that stories play in individuating the very material evidence that it is evidence for. The upshot of this section, in addition to a refined view of material evidence, is that I will be in a position to articulate a more precise concept of coherence, which will then be defended, in the final section, against the circularity worry that I opened this chapter with.

### 3.4 Timecapsules

This section is about the “stuff” from which we infer the past. Terms used in the literature are ‘material evidence,’ ‘artifacts,’ ‘fossils,’ ‘relics,’ and, perhaps most commonly, ‘traces.’ Typically, authors use these terms to mean something like downstream or distal effects from some past cause. In this sense, we are, so to speak, swimming in a sea of traces, for everything is a trace of some past cause or other. This seems to be how Chapman & Wylie (2016); Cleland (2001, 2011); Turner (2005), for example, use the term. Currie (2018) differs slightly in that for him “trace” is a success term. So we are swimming in a sea of potential traces, but traces

are only things that are connected to their cause by way of some known midrange theory. For example, a fossil is a trace for Currie, because we have a midrange theory, in this case provided by the science of taphonomy, that connects present day fossils with their causes, viz. long ago critters. Note, however, that in the later Currie (2019a), traces are defined as distal effects.

I do not disagree with this characterization, but I wish to shed light on different aspects of the endeavor to know what happened in the past. More specifically, I want to know how we go about identifying the “stuff” from which we can infer the past when we do not already know, or at least suspect, what happened. I’m interested, that is, in the individuation conditions of traces, and what it takes for a presently observable state to bear information about past states. More specifically, I’m interested in what I take to be a widespread feature of historical investigations, where the theory that makes a trace meaningful of the past is not known independently of the story of what happened. For example, the theory of how fossils form from bones is epistemically independent, or seems so, of paleontological reconstructions.

Moreover, it seems that the way in which traces are discussed in the philosophic literature is ambiguous, and so obscures what I take to be an important distinction in the practice of historical science. I think there are two basic concepts important for understanding how evidence is used to reconstruct the past that have not been explicitly disentangled. (I very briefly introduced this distinction in Ch.2.) The distinction is based on two simple observations. The first observation is that most processes leave marks; in fact most leave many. Marks are easy to produce; hit something with a hammer, write on a board, walk through mud, etc. Committing the perfect crime is hard, for this very reason; just about everything one does leaves some mark or other. When we want to reconstruct what happened, we need to find the appropriate marks, which we can then use to infer what happened to generate them. In other words, marks are the primary source of evidence needed to reconstruct past events; they are the evidential basis for inferring what happened to make or produce them. There is some subtlety here, for some marks are taken to be the result of some event that occurred, as in my having struck something fragile with a hammer. Other marks are taken to indicate not so much something that

happened, but the prevailing state of some target system. Both are crucial to reconstructing what happened. Marks that indicate the state of a system help us identify the states—as well as their sequence—that constitute the history of the system. Process and state-indicating are not exclusive categories; some marks can do both. And the same object or system can bear multiple marks.

The second observation is that, although most marks are lost as subsequent processes overwrite the marks made before, some marks are preserved. When glaciers advance, they can carve out valleys. When they recede, they leave behind terminal moraines, piles of pulverized debris that mark the extent of a glacier's migration. Such valleys and moraines are marks. This kind of preservation of marks, however, is uncommon. Preservation very rarely leaves the mark unchanged; preservation is a process that leaves marks, too. Dinosaur fossils are an example of how preservation alters initial marks. When organisms die, if the conditions are right, the remains form fossils. The dead organism was the mark. But what we find are not even the organism's bones, strictly speaking, for fossils are not made of bone but of rock; subsequent chemical processes convert bone into rock, irreversibly changing the organism's remains. This is the norm: we never, or very rarely, find marks.

“Trace” as used in the philosophic literature is ambiguous in that it is used sometimes to refer to the thing observed and sometimes to refer the mark. It is also ambiguous in the sense that it is sometimes used to refer only to observables that have been picked out by some theory as being relevant (Currie (2018) tries to use it this way), but it often also refers to observables that are there but are not yet so picked out. It conflates the thing observed with the mark, which is something inferred. So, for example, discussions of fossils will describe them as bones, when they are not. Now in the case of fossils, this is pretty harmless, for the link between fossil and the mark, a bone, is not controversial. In the kinds of cases that I am interested in, however, such as in geochemistry and astrophysics, conflating the observable with the original mark obscures an essential and very difficult part of the investigation. A substantial portion of the work required in historical investigation is devoted to the practice of inferring marks from that

which is preserved, of unwinding the subsequent alterations to the mark that the preservation and intervening time have imprinted on it. And then the work of using the marks as the basis of investigating the dynamics involved in the target system can begin. It is this unwinding part that is left out of the philosophical expositions of historical science and inattention to it, I think, is what makes a view such as Cleland's plausible, for it leads one to take the connection between present observable and past cause as less tenuous than it really is.

In Ch.2 I distinguished the mark from the present observable, which I called a "trace." I did so because a reviewer insisted that I use the more common word. In what follows I will stop using "traces" and switch to my preferred term, "timecapsules." The name of the game, so to speak, is to reason about marks, but what we find instead are timecapusles—**marks that have been preserved and, thereby, changed.**

Insofar as rocks are so durable, they can make for excellent timecapsules. Not all timecapsules are encased in stone, however. The big bang produced a timecapsule that fills the entire observable universe. The photons now collectively called the cosmic microwave background radiation are streaming all around us and in every direction. To the extent that organisms today bear the marks of their lineage's evolutionary past in their DNA and morphology, they, too, are timecapsules. These photons are a timecapsule that was produced a few hundred thousand years after the big bang and indicate something about the distribution of matter and energy in the early universe, that it was remarkably homogeneous. In fact, photons have a black body spectrum, which indicates that radiation and matter were in local equilibrium at the time of emission (Aghanim et al., 2020). The CMB is a surprisingly rich timecapsule, also revealing, for example, that there are correlations in the CMB spectra between regions of space that could not have been in causal contact without, say, a period of exponentially accelerated expansion—i.e. an inflationary phase of expansion.

Fossil timecapsules can also bear marks from the individual organism's life history, such as patterns of wear and tear on dentition, stomach contents, coprolites—that is, fossilized feces—and so on, not just the genetic history of the lineage to which it belongs. Much like the way



human remains and artifacts are timecapsules that reflect more complex dynamics, fossils can do so as well, by being, say, situated within a nest containing the organism's young of various ages, as was the case with the dinosaur, *Maiasaura* (see Horner & Makela (1979) for the original publication of this finding, or see Woodward et al. (2015) for a recent assessment of the decades of related paleontological research on *Maiasaura* since). Cycles of stress and strain can leave marks in mechanical systems as fatigue cracks grow in, for example, jet engine turbine blades. Each cycle of stress leaves a pattern of striation within the metallic lattice structure as fatigue cracks form. And if any pieces of the blades survive a crash and are found, they are timecapsules. The presence of striations can then be used to infer how many stress cycles—i.e. take-offs and landings—the aircraft underwent during the lifetime of the blade.

The endeavor to investigate history, then, involves two conceptually distinct, if difficult to separate in practice, tasks. To access prehistory, we must discover timecapsules that preserve marks. But since the processes responsible for generating the timecapsule typically alter the mark, scientists must first unravel these processes in order to infer what the mark was. Only then can they infer the processes and states that generated these marks. In Ch.4 I discuss an instance, in the investigation of the origin of the Moon, in which failure to properly unravel a timecapsule occurred, and how this leads to a misidentified mark and then to a faulty inference regarding the historical reconstruction.

The notion of building a timecapsule in order to commemorate some contemporary achievement or happening that people take to be significant has been around for a long time. Many years ago, my hometown built a new library and wanted to commemorate the occasion by sealing an extremely sturdy, air-tight, fireproof, and etc., box within the foundation of the new building. The box was to contain items, voted on by the entire community, that represented what life was like for us at the time and what we valued. It also included a letter explaining the items and why they were so enclosed. The idea was that at some time in the far-distant future, archaeologists would be able to dig the box out of the ground and learn what everyday folk were like and what we were interested in as a culture.

This notion of a timecapsule is a useful starting point, but has some problems. Could an archaeologist in, say, 10,000 years open up such a box and tell what it was for. One of the problems with deciphering a timecapsule is similar to the problem archaeologists have when studying graves. The dead don't bury themselves, and so a grave-site often contains artifacts that potentially tell about sociocultural aspects unrelated to the person's death. But the meaning of a grave is notoriously difficult to decipher. Similarly, upon finding my town's timecapsule in the far future, archaeologists might wonder whether it was a special religious shrine. Did the people who created it worship these items? What was each item used for? And what would an archaeologist make of the letter of explanation enclosed in the box? Unless there was some continuity of language from then until the future, how could they decipher the letters? These problems all stem from the fact that the objects have an intended meaning, but this only exists in the minds of those of us who made the time capsule. Without knowing why the objects were placed in the box, and without already knowing what those items were used for, the scope of reasonable inferences open to future archaeologists is poorly constrained and, therefore, the inferences may amount to little more than wild speculation, especially with greater temporal and cultural distance. And yet, the real value of the timecapsule does not lie in the unambiguous meaning of the items therein, but in their preservation, for the items enclosed tend to be the kinds of things that ordinarily are easily destroyed.

Moreover, what makes the interpretation of the items difficult is the fact that they were not em-placed by a natural dynamical process; the processes involved in their creation or subsequent use are not responsible for their presence in the timecapsule. Instead, they are present in the timecapsule because they feature in a story that my community was trying to tell. A timecapsule is a collection of artefacts that tell a story. This connection between objects and stories is illuminating of the methodology of scientific investigations of the past.

One of the most fascinating aspects of antiques is not in the object itself. Rather, what can be so fascinating about antiques is the story that accompanies them, whether we know the story or are merely imagining what the story might be. And this story often includes the particulars

of time and place, who made the artifact and why it was made, along with why it was valued enough to be preserved. For timecapsules, in the sense I've given, of marks from past, non-human, processes, that have been preserved, perhaps the most important elements of the story that we hope to be able to infer, are how they were made, and why they have the properties they do and not other properties. Additionally, stories of timecapsules that have interesting accounts of how they were preserved, especially when the preservation of a timecapsule was itself a highly contingent sequence of events, can be especially compelling. This connection between timecapsules and stories is nice, for as I suggested in the previous section, timecapsules are individuated under a provisional historical narrative. And so, it seems, the connection between stories and timecapsules runs deep, indeed.

Timecapsules, then, license inferences of a sort that anchor, and, thereby, constrain a story of what happened to some system. Moreover, colloquial timecapsules typically contain many artefacts that are unified by the story they are meant to tell. A timecapsule is a set of objects that are mutually connected by a story. And what the story does is tell not just how the timecapsules were formed, but how they are interrelated, and how they would be different had the confluence of processes or had the state of the target system been different, either at the time the timecapsule was initially formed, or had the subsequent history of the system been different. The endeavor to know the deep past is one in which scientists seek to individuate ever more elements that constitute a timecapsule, and which anchor enough facets of the story of what happened to make a target system evolve along the trajectory that it did. I will sometimes have need of referring to individual elements within a timecapsule, I will call these "relics," though the term trace would work as well, so long as the relic or trace is distinguished from the mark. Historical scientists, then, are in the business of "building timecapsules," of identifying sets of relics that, together, constrain the story that binds them, so to speak.

Turning back to the previous section, I want to consider further what stories individuate. As I argued previously, stories individuate central subjects. But stories do more than this. Recall that one of the challenges of historical inquiry is not that we have very few marks from

past events to find or choose from. Rather, the problem is quite the opposite, for marks are everywhere, if only we knew how to interpret them. Some difficulties arise because few marks are unambiguous, being often the product of many different processes and events. In order for an observation of an extant feature of a system to count as a relic, and so be included in a timecapsule we are constructing, we need to recognize it and have some way of inferring what the mark was that has been preserved, and then what happened to produce the mark. Individuating or recognizing a relic, then, requires substantial background knowledge. And differences in accepted background knowledge can result in investigators disagreeing about whether something constitutes a relic at all. Relative to one set of background knowledge, an observation may count as an important relic, while, relative to a different set—with, say, different working hypotheses—an observation may simply be set aside as unrelated to the history being investigated.

That we need substantial background knowledge in order to identify a relic is uncontroversial. It is worth stressing, however, because some relics strike us as so obvious that we can lose sight of the epistemic issues that I am concerned with here. Suppose, for instance, that we come across a large fossilized skull while on a hike in the desert. This is so obviously a relic that we would not fail to recognize it as such. But, like all relics, simply being an interestingly shaped rock or being somewhat rare is not enough. Our immediate thought that a fossil is a meaningful relic, telling us about an organism that lived here in the distant past, is based on substantial background knowledge. If we really had no relevant background knowledge, then the fossil would simply be a rather curiously shaped rock, as was the case before the fairly recent development of theories of fossilization, etc. We might then wonder what could have produced such a strange rock, and, of course, this would be the first step toward acquiring the background knowledge necessary for inferring what could have produced such a mark.

Even examples of relics that seem so obvious, then, show that crucial to the recognition of a relic is understanding that relics, themselves, are not always identical to the original mark. For instance, marks are seldom completely preserved. In the case of finding a skull-shaped rock,

we conclude that there was once more than what we now see; it was a complete organism, not unlike animals we see today, with hard, bony parts, that were (incompletely) preserved, as well as much else that was not preserved by fossilization. So the reason that a fossil is easily recognized as a relic is that we know that: a) the processes involved in preserving a mark usually do not leave the mark as it was formed initially. Rocks in the shape of large, ferocious skulls do not occur in nature except when formed by a process that begins when an organism is buried under certain conditions and certain kinds of its tissue then undergo a chemical exchange process, thus preserving some anatomical features in stone. And we also know that b) what is preserved is seldom complete. That is, we do not simply marvel, thinking that there was once an organism that consisted solely of a giant skull. Our experience as biological organisms ourselves is important and we—perhaps too easily, at times—infer things about the past based not only on what we find in the present, but also on the basis of our background knowledge of the possibility space of how certain features we might find could have been formed, even sometimes pre-theoretic background knowledge. The individuation conditions of relics are provided by either a story of how not only certain states of a system could have been formed, but also how things should be in the absence of the relevant processes, or by an element of surprise in the sense that a surprising or unexpected or atypical feature may well be the result of processes not countenanced by the story we already have. And when we have this kind of provisional story, then we are able to recognize the difference between what is typical of a given system, and what is the result of other processes.<sup>29</sup>

The point here is that relics, the lines of evidence that support claims about the past, are individuated by those stories. An observation of a current state is not informative except in relation to a body of background knowledge according to which observations are expected or unexpected. In some cases, this evidence is individuated by midrange theories that are independent of the hypothesized story. But often the story itself does this work of individuating its evidential basis. Often, the story of the marks comes first, and this then guides the search

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<sup>29</sup>Often cases of surprise or atypicality result from departures from known or provisional story-lines, and so call for full blown stories.

for relics to combine in order to build a timecapsule that further enriches the story. In the origin of the Moon case, for example, the hypothetical giant impact individuates certain geochemical marks, it tells us what geochemical signatures to look for and how to interpret them. Without a story like this, there's no reason to do the coupling required to make use of the short-lived chronometers. At the same time, though, there is another important facet of studying the past, for investigators have to "subtract off" subsequent goings-on that left additional marks on the signature and so forth. This aspect of investigations is crucial and often is the most challenging. I will illustrate this further in Ch.4. I also briefly touched on this earlier when discussing the cosmic microwave background that is central to cosmology. In that case, the last 30 years have consisted of studying the relic of the CMB in order to better understand the mark that it represents.<sup>30</sup> The CMB is individuated as a relic by the big bang story, but in order to use it to further constrain or enrich the story, cosmologists have had to do much to understand which features they observe today are due to subsequent processes. Moreover, some of the observed properties of the CMB are actually parochial, due to our motion with respect to the CMB. But very often, this unwinding of relics to marks is not independent of the provisional story of the mark-making processes.

The potential constraints provided by path-dependence is related to timecapsules and the sequence of turning points in the target trajectory. Since turning points mark shifts in the dominant dynamical regime, the goings-on capable of producing such changes are good candidates for timecapsule production. Some timecapsules indicate the state of the system at a time. These can be used to get a sketch of the dynamical processes governing the system at a time. This isn't sufficient for a full story, though, because the full story accounts for why the system evolved in the way that it did and not other ways. In this way, it is possible, though I think unusual, for timecapsules to enable us to determine the sequence of states that a system occupied, yet without telling us much about why the system occupied those states. In such a case, we might know what happened without knowing why. But because historical trajectories

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<sup>30</sup>The CMB is also a timecapsule in the sense that it is really a large collection of relics.

are often so sensitive to the sequence, timing and intervals between turning points, a simple timeline can play a powerful role in constraining the space of possibilities for what kinds of processes could have been involved. Constraints on possibilities provided by timelines are not only useful in criminal trials. Attorneys often place great weight on a timeline because they want to show that the accused could or could not have committed the crime within the spatio-temporal constraints imposed by the timeline. Likewise, scientists use timelines to constrain theorizing. The anomalous lead age of the Earth, coupled with our understanding of the timing of accretion—the process whereby solid grains form in the dust and gas disk surrounding new stars and then grow to form asteroids and planets—was a crucial constraint that led to serious consideration of the giant impact origin of the moon. As I mentioned earlier, the lead age is ambiguous, but it was enough at first because the Earth cannot be younger than everything else, for planets form early or not at all.

Note, however, that relics need not be associated with a turning point. Some are useful insofar as they just indicate the state of the target system at some time. An example of this, and which I will discuss in more detail in Ch.4, is the the oldest known mineral grain on Earth. This relic is a zircon grain, found in Jack Hills, Australia, and it licenses a remarkable inference (Valley et al., 2002). Analyzing the lead isotopic ratio of the zircon grain, we know that it formed 4.404 billion years ago. On one hand, this might seem surprising, since the Earth is much older than that. How is it that the oldest rock is nearly 100 million years younger than the planet? The reason is that the Earth is such a dynamic system, that the none of the earliest rocks that formed as the Earth slowly cooled have survived. The Earth's earliest crust has been completely recycled, so the age of the oldest grain is not particularly interesting for my purposes here. What is interesting, is this age coupled with another isotopic ratio in the grain that licenses a surprising inference. The oxygen isotopic ratio of the grain indicates a general feature of the prevailing conditions on Earth at the time of the grain's formation. Recall from Ch. 2 that the Earth was completely molten, even largely vaporized, by about 4.49 billion years ago, in the aftermath of the collision and merger with Theia. What the Jack Hills zircon grain

tells us, though, is that by 4.404 billion years ago, the Earth had cooled so much that liquid water was present on the surface. This remarkable finding is not in conflict with the Giant Impact story, for there is enough time for the Earth to sufficiently cool by 4.404 Ga. But this is just enough time. If additional zircons are found that are even older, there will be a significant conflict, as there won't be enough time for the Earth to cool.

In physical cosmology, the initial post-Big-Bang sequence plays a crucial constraining role. In this case, the sequence is based on few timecapsules, but the CMB, coupled with plausible dynamical assumptions enables cosmologists to construct a sequence of the Universe's early thermal evolution. The reason that inflation was proposed was that there was not enough time, given the known dynamical processes, for the Universe to evolve to yield such a uniform microwave background radiation. Unfortunately, the CMB is ambiguous in the sense that it is insufficient to select neither among the many possible formulations of inflation nor from non-inflationary rivals. In this case, it turns out that the concept of inflation is extremely rich and flexible, and so theoretically useful, but so far has few empirical consequences beyond what cosmologists already know independent of inflation. The same is true of alternatives such as string cosmologies, though the lack of empirical consequences is perhaps worse. I will return to this topic in §4.3.

Timelines, therefore, if sensitive enough, can serve to turn state-indicating timecapsules into process-indicating timecapsules in virtue of the fact that the timeline constrains the possible processes involved. Some processes are ruled out for they would take too long. Others are ruled out on the basis of the sensitivity of the sequence. In this way, path-dependence is a useful resource. While determining the sequence of states that make the system's trajectory can sometimes be done without knowing the dynamical details responsible for the trajectory, a full story must include these details. And, indeed, understanding the details of why the system evolved as it did is crucial for, again, stories, just like theories, earn their keep by enabling epistemic progress, that is, by enabling epistemic access to more and more of the causal structure of the world.



In addition to disambiguating the notion of trace, this discussion of timecapsules, relics and marks raised two points relevant to the overall aim of this chapter, viz. to clarify the notion of coherence at stake and to defend it against a circularity worry. The first point is that the evidential lines that support stories of what happened are often individuated by that story. *Prima facie*, this makes the circularity worry sharper, for how can the evidential lines provide confirmational boost when they are picked out and made evidence by the story they are supposed to confirm? It seems that boosting credence in a story on the basis that it is the best explanation of the relevant lines of evidence is an instance of double counting. Of course the story explains that evidence, for it was designed to. What reason do we have, then, to think that the story isn't simply a just-so story, that picks out certain features of the world and explains them, but without making real empirical contact, so to speak, with the world such that the story is amenable to empirical overthrow. This is the just-so problem that will be addressed in the next section.

The second point highlighted in the present section concerns unification. The unifying nature of narratives has been oft discussed in the philosophical literature. The emphasis of this discussion is often on the explanatory power of narratives, how they provide unifying explanations. The discussion here, differs slightly. Stories have the form of unifying explanation, but the emphasis is not on explanation. Instead I have emphasized the notion of individuation of a collection of mutually constraining relics. In the next section I will use this idea, coupled with the findings earlier in the chapter, in order to explicate a refined understanding of the kind of coherence involved in story-mediated reasoning and then show how it undermines the circularity worry.

### 3.5 Coherence Found: Story-mediated Measurement & Epistemic Leverage

The aim of this section is to put all of the insights gleaned from the earlier sections together to characterize the form of coherence relevant to stories of the past and to address the worry about their seeming reliance on coherence for justification. The worry seems to be that reliance on coherence for justification is problematic insofar as coherence doesn't track truth, it isn't truth conducive. I've noted earlier that the issue is that just-so stories seem to display a kind of coherence, in the sense that they "hang together." The present section will proceed as follows: first I will sharpen the worry about coherence by reviewing work in formal epistemology that demonstrates that coherence, qua "hanging together," does in fact fail to be truth conducive. But, second, the notion of "hanging together" fails as an adequate explication of the form of coherence relevant to justly scientific stories. Using the insights accumulated by way of the discussions of the previous sections, I will build a stronger notion of coherence. Then, third, I will show that this form of coherence is truth-conducive. (In Ch.4, I'll turn back to the case study, and illustrate this notion of coherence in more detail.)

In the formal epistemology literature, the issue of the truth-conducivity of coherence has been pursued and seemingly resolved (see, e.g. Olsson (2005); Bovens et al. (2003); Schupbach (2015)).<sup>31</sup> It seems that on a natural way of formally representing coherence holding between a set of propositions, the presence of coherence does nothing to boost credence. A typical scenario considered involves the testimony of two unreliable eye-witness reports of a crime. Owing to the eyewitnesses being unreliable characters, their individual testimonies do little to boost credence in the hypothesis, say, that Moriarty did it. But, the testimonies are cooked up in such a way that they hang together, in the sense that they agree or that they provide complimentary information that, when taken together, seem to greatly increase the likelihood that Moriarty did. It is further stipulated that the two witnesses have not colluded—their testi-

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<sup>31</sup>Insofar as the present work makes no use of the Bayesian formalism, I will leave the formal details of this work aside. For a brief overview of this issue, see Olsson (2017).

monies are independent of one another—and that, owing to their unreliability, their individual effect on the probability of the hypothesis is nil or very small. What’s needed, then, is to show that if their testimonies hang together in a way that seems very unlikely, then it should be the case that the probability of the hypothesis, given both testimonies, is greater than the probability that results from taking the sum of the individual probability boosts. Unfortunately, the fact that the two testimonies hang together fails to boost credence beyond the sum of what they do individually; coherence fails to provide confirmational boost. There seems no way for the hanging togetherness to add additional support, and this accords well with the intuition that conjectures spinning in the air can hang together and yet there’s no reason to think they are, thereby, true.

But what this “hanging together” amounts to is too vague and, from what I’ve shown already in this chapter and the one prior, corroboration of independently known evidential lines fails to sufficiently capture the relevant notion of coherence. In search of a clearer sense of what coherence is, I considered the roles that a story of the past can play and I considered whether stories are eliminable, in the sense that they are merely the rhetorical or pedagogical devices used to convey a time-ordered series of independently known research findings. I showed that the answer is “no.”

The initial worry with coherence was that conjectures freely spinning in the air can exhibit a hanging togetherness. But looking at the role a justly scientific story can play in licensing measurements, it’s clear that the freely-spinning worry misses the mark, for the lines of evidence are empirical, and the story that, say, the Hf-W and U-Pb evidential lines are evidence for does not guarantee that they will provide agreeing support. Moreover, in the formal work (mentioned above) purporting to show that coherence isn’t truth conducive, the lines of evidence (the testimonies) are stipulated to be independent from each other. This stipulation, referred to as conditional independence, fails to represent the form of coherence that is relevant to stories of the past, for the lines of evidence are not conditionally independent: the Hf-W evidence depends upon both the giant impact hypothesis as well as the U-Pb evidence. The pro-

posed hanging together simply is too weak, coherence has to be more than corroboration from independently known sources.

An alternative articulation of coherence that is less vague than “hanging together” and that is more in accord with this failure of conditional independence is due to Whitehead (2010). Whitehead was concerned with conceptual schemes, rather than stories, and defined coherence thus:

The fundamental ideas, in terms of which the scheme is developed, presuppose each other, so that in isolation they are meaningless... they cannot be abstracted from one another.

This goes some way towards capturing the notion of non-detachability discussed earlier in this chapter as well as the notion of theory and story-mediated inference. This notion of mutual presupposition is close to what I’m after. This issue of non-detachability came up, above, in the discussion of a case due to Ereshefsky & Turner (2019). Recall the example of the narrative of how Cecilia bought some apples. In that example, the story of how Cecilia came to buy some apples amounted to a time-ordered sequence of independently knowable occurrences that culminated in Cecilia at the store and deciding to buy apples. The point I made above was that the narrative structure of that story does not really do anything essential. Here I want to point out that stories like that example do, indeed, hang together, but they actually display a very low degree of coherence in the sense that I’m after. Instead, coherence must involve a relation according to which the elements of a story are not knowable in isolation; they are known together or not known at all for they involve a relation of mutual presupposition.

What the Whitehead conception of coherence lacks, however, is a connection to the world, so to speak. That is, one might rightly worry that although this relation of mutual presupposition is harder to come by than a mere listing of time-ordered facts that stand independently of one another, it still seems plausible that such stories can be freely spinning in the air. And, indeed, this is precisely what’s problematic with adaptationist stories, for example, in evolutionary biology. They display this kind of interdependence, and even purport to be about

empirical matters. Yet their connection to the world, in the sense of their being capable of empirical overthrow, is illusory. There's something about the mutual presupposition relation in these cases that guarantees that no evidence could be brought to bear on them.

So mutual presupposition as a feature of a story is insufficient to make demands of our credence. What's needed is an evidential or empirical connection. Such a connection would answer the worry above that coherence isn't truth conducive.

In Ch.2 and earlier in this chapter, I think I've laid the groundwork for explicating how this goes. One of the treasures that I found earlier was the notion of unification, that stories are powerful tools for bringing together disparate lines of evidence. Indeed, the unification of disparate evidential lines is often considered to be an important feature of theories in all areas. But why does unification seem to be truth-conducive?

Successful theories in science do tend to be unifying. The problem, though, is that unification stands in need of some further justification if it is to be a mark of truth. What is needed is some kind of principle according to which nature is simple and unified, such that true explanations are unifying. But, of course, misleading associations seem possible, and so an explanation that unifies disparate traces might well be unifying unrelated phenomena. Again, this is the problem of underdetermination from Ch.1. What's needed, then, is some empirical ground for thinking that misleading associations are rare, i.e. something like the asymmetry of overdetermination thesis. Indeed, for Cleland and Currie, unification of disparate lines of evidence has confirmational power in virtue of the asymmetry of overdetermination. Recall that the asymmetry protects us from being fooled by evidence that that seems to fit some common cause story but actually is misleading. But insofar as I've demonstrated that the asymmetry of overdetermination is false, I've undermined that avenue for taking unification as providing justification.

Moreover, taking unification of independently known lines of evidence as confirmation of the unifying theory is double counting of evidence. An explanation's ability to unify independently known phenomena counts as a successful test of consistency with background

knowledge, but such unification does not provide confirmational value above that. Taking it as providing further confirmation is problematic in that the hypothesis was formulated so as to unify the known evidence. And so this kind of unification is nothing more than the hanging together that has been demonstrated to be incapable of providing further evidential support.

What, then, is the value of disparate lines of evidence? The answer is in another issue that came to light earlier in this chapter. Justly scientific stories don't merely unify independently known observations, they in fact individuate many of the lines that are then evidence for the story. The lines of evidence themselves depend, for their status *qua* evidence, on the story they are evidence for. Such is how theory and story-mediated inference works. I demonstrated this in Ch.2, where I showed that the Earth's Hf-W ratio has very little meaning apart from the story of core-mantle formation as well as the giant impact hypothesis, which license coupling Hf-W to U-Pb. The sense of coherence I'm developing here unifies, but in the sense that it is a relation of co-dependence between elements of the story, on the one hand, and empirical evidence on the other. Moreover, my claim is that this kind of arrangement of mutual co-dependence between both elements of theories and of stories as well as their empirical evidence is a ubiquitous feature of successful scientific reasoning.

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The question is then how is this not viciously circular? Isn't it problematic that the evidence for stories presupposes the stories they are evidence for? If one holds a view like that which I argued was lurking behind the worry, in Ch.1—that historical sciences need an alternative inferential method, what I there called an impoverished view of both experimental and historical science—then sure what I'm describing here is viciously circular. On that kind of view, the highest standard of evidence consists in comparing predictions derived from a theory with

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<sup>32</sup>To be sure, independent measures of a quantity converging on a common value is powerful evidence. I'm not here claiming that independent lines of evidence aren't important. There are. My point is that this is not the only, or even the most important form of evidence. Moreover, the sense in which converging independent measures are independent is a matter of difference of emphasis. For example, Harper (2011); Smith & Seth ("in press") both stress the importance of complimentary measures of fundamental constants and how this is a very powerful form of evidence. For them, alternative measures are independent if they do not presupposes the same subset of elements within a theoretical framework. For my purposes, such complimentary measures are not independent lines of evidence, for they still depend, for their status *qua* evidence, upon the theory that they are then evidence.

independently measured or observed results. On such a view, it would be rather worrying if not only the predictions, but also the observations, depended upon the theory being tested.

There is a considerable literature criticizing this kind of (impoverished) picture of scientific testing. Many, perhaps most, philosophers at least claim to completely reject such a picture and insist upon dropping discussion of scientific method. I think that's going too far. There is something of an inferential method that is quite ubiquitously employed and is more general than the so-called experimental method, viz. theory and story-mediated measurement. The experimental method, sometimes called hypothetico-deductivism, is very limited in that it can really only support the claim that an observation is or is not consistent with some theory or hypothesis. But it seems that evidence can bear positively, in the sense that some observations do more than indicate that a theory is possibly right, they seem to make more demands of our credence than consistency tests can.

I now turn to arguing that this *prima facie* circularity—that observations are individuated by, and so presuppose, the theory they are evidence for—however, is ineliminable, but not vicious. Instead, it is a sign that the evidence is not just a consistency check, but, if corroborative, makes demands of our credence. That is, coherence, as a relation of mutual presupposition that includes the relevant evidential lines is truth-conducive.

The case study of Ch.2 has already demonstrated the key point that the story-mediated Hf-W measurement of the timing of the giant impact was capable of disagreeing with the U-Pb chronometer that is presupposed in the measurement. Indeed, the Hf-W system was selected because the U-Pb chronometer is thought to be less reliable in the case of the Earth's core formation. Moreover, the Hf-W chronometer does disagree. What this shows is that by presupposing the giant impact, which licensed calibrating the Hf-W chronometer with the U-Pb chronometer, does not guarantee that the Hf-W measurement of the timing of the giant impact would ultimately fit with the hypothetical story. Story-mediated reasoning, in this case, doesn't "bake in" a consistent measurement outcome. It could have gone wrong. That it didn't, that the resulting date fits very well with the other elements of the overall story of the Earth's accretion

and early history also shows that this kind of reasoning is subject to empirical overthrow. It's not a circle of ideas freely spinning together in the air. Scientists learned something from the measurement that they did not know before. If this were a case of vicious circularity, then presupposing the story to make the Hf-W measurement could only tell scientists what they've already presupposed, and so nothing would be learned.

One line of evidence is suggestive, but isn't enough. It would be nice to have a variety of evidential lines. As I mentioned above, it's important to have disparate lines of evidence, which is why unification is so often recognized as important. Indeed, Smith (2001); Smith & Seth ("in press"); Harper (2011) have both stressed that in experimental cases, an important strategy is to seek complimentary measures. Complimentary measures are alternative measures of the same quantity that presuppose different aspects of the theory being tested. With but one means of measuring, e.g., a fundamental parameter, scientists rightly worry that the result cannot be trusted as well as if it could be cross-checked by an alternative approach that does not presuppose all of the same elements of the relevant theoretical framework. But in historical scientific cases such as the origin of the Moon, it's often not possible to develop complimentary measures of the same aspect of the story. In such story-scientific cases, what typically has to be done is to target other aspects of the story for further investigation. In so doing, additional story-mediated inferences are made that hopefully further refine or extend the initial story sketch that was presupposed. Crucial to this ongoing process is that different aspects of the story sketch are presupposed. In this way, these further inferences act as a kind of cross-check on the inferences that came before, similar to the use of complimentary measures in experimental work. The demand that the story be fleshed out in additional detail is an important test insofar as this work generally engages different aspects of the story. By doing so, the story is further tested, for, again, the resulting is not guaranteed ahead of time to be consistent. (In the next chapter I will illustrate this by showing how the investigation into the origin of the Moon has progressed to answering further questions beyond the timing of the giant impact.)

What happens in this ongoing process of delving into deeper detail is the answer to what the



nature of coherence is that I've been seeking. In Ch.2, I used the term "coherence of evidence" to designate an interesting feature of successful theories and stories. There I was referring to the way in which the lines of evidence, when taken together, have greater probative value than they would if their individual (story-independent) values were summed together. I can at last answer the circularity worry and say why that is and justify my claim that, when scientists can successfully presuppose a story to individuate new measurements that further refine the story, the story accrues confirmational boost.

The metaphor developed earlier in this chapter will be helpful here. We want the story to build a timecapsule by individuating many different kinds of relics. Each relic will presuppose some part or parts of the story, but not all of it. So the relics are unified by the story, but there's one final and crucial aspect to my account of coherence of evidence that delivers the additional probative value that the "hanging together" account of coherence cannot. In addition to presupposing different aspects of the story and so testing different combinations, the relics in the timecapsule are situated within a relation unto each other. The relics depend upon the story for their status as evidence, but they also are placed within a relation of mutual constraint. That is, the relics are made, by the story that individuates and unifies them, to be mutually informative.

In the next chapter, I will return to the case study and illustrate this notion of mutual informativeness between relics with several more pieces of the story of the early Earth and Moon. From Ch.2 though, I've already illustrated this relation of mutual informativeness with the relics of the Earth's U-Pb and Hf-W ratios. The giant impact story licenses using long-lived U-Pb to constrain the start time of the short-lived Hf-W chronometer, which then refines the estimate of the age of the Earth's core. In the next chapter I'll add to this by showing that the relation of constraint is really mutual, that the short-lived chronometer constrains and recalibrates the long-lived chronometer. This would seem like a strange way to reason, for, recall, the parent isotope of Hf-W is extinct and so it needs to be coupled to a long-lived chronometer if it's to yield ages. How could it then be used to recalibrate the start time for the long lived

chronometer? The answer is going to reveal that there's something not quite right about my explication of the case in Ch.2. There I described the U-Pb chronometer as being story independent. But, as I'll show in the next chapter, it too is a story-dependent relic. This re-calibration of the start time of U-Pb then opens the epistemic window to further details about the dynamics of the early Solar System at times thought to be inaccessible because they happened too early and too fast for the low resolution U-Pb chronometer to detect.

I will now close by showing that this kind of coherence of evidence, of co-dependence between lines of evidence, is truth conducive. The argument draws on the work of Myrvold (2003, 2017, 2020). Myrvold is concerned with a different, but related issue. He is concerned with the epistemic status of the theoretical virtue of unification. In particular, Myrvold is worried about taking the explanatory unification afforded by a hypothesis or theory as providing confirmational boost. A hypothesis, if true, might well explain and thereby unify various phenomena. The problem is that such explanatory power does not increase the probability that it is true, or at least not without supplementing the reasoning with metaphysical presuppositions regarding explanatory power or the simplicity of nature or the like. This issue is very like the issue with coherence. The unification of disparate lines of independently known evidence has no additional probative value beyond the sum of the values of the independent evidential lines.<sup>33</sup> Nevertheless, unification of disparate evidence seems very important, and so stands in need of justification insofar as it affords confirmatory boost to theories or hypotheses. What Myrvold shows is that explanatory unification, in general, is not truth-conducive, but there is a special form of unification that is. He calls this form "mutual informativeness unification (MIUnification)." The argument for the truth conduciveness of MIUnification is formulated formally, using a Bayesian framework. I'm not concerned in the present work with the particulars of the Bayesian formalism, so I will not go into the argument in detail, instead I'll summarize the result.

Myrvold begins by deriving a formal measure of informational relevance between lines of

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<sup>33</sup>Note, however, that the emphasis on non-independence of evidential lines is mine and not present in Myrvold's work.

evidence for some hypothesis. He then defines a measure of the extent to which the hypothesis unifies the lines of evidence, which turns out to be proportional to the informational relevance, conditional under the hypothesis, the evidential lines have to each other that they do not have otherwise. Myrvold then shows, by way of application of Bayes' theorem, that the degree of evidential support conferred on a hypothesis by multiple lines of evidence equals the sum of the degrees of confirmation conferred on the hypothesis individually, plus another term. And this additional term is the degree to which the hypothesis makes each evidential line yield information relevant to another.

Myrvold is primarily concerned with cases of unification in the history of physics, where he shows, e.g. that the observations that led to the stalemate between the Copernican and Ptolemaic hypotheses concerning the structure of the Solar System actually favored the Copernican hypothesis because it makes the observations of the orbital parameter more mutually informative. What's interesting about this result is that the choice between the two systems was debated for so long and was largely waged on considerations of the simplicity of nature as well as on vague appeals to one or the other offering a more unified view. But such arguments then, and now, have a dubious character and seem circular. Occam's razor type arguments didn't render a clear verdict in this historical case, nor do they generally. They are examples of attempts to make metaphysical intuitions into epistemological tools, which they are not.

Myrvold is concerned with the evidential unification provided by theories, as opposed to stories of particular events (recall the loose distinction I drew in Ch.2 between theories and stories). But I have shown that stories of particular events can play the role of licensing measurements and so, too, can they individuate relics and place them in a relation of mutual informativeness, or mutual constraint. So it follows that what I'm calling coherence of evidence is a form of what Myrvold calls MIUnification. The probative value of a timecapsule, a collection of relics, exceeds the sum of their individual values. And note, too, that what looked like worrying circularity—insofar as the relics presuppose the story they are evidence for—is actually an important epistemic feature of the reasoning, for it ensures that the mu-

tual constraint between the relics really is conditional on the hypothetical story, as required by Myrvold's MIUnification argument.

Moreover, I claimed earlier that theories and stories earn their keep inasmuch as they do epistemic work. The epistemic work that they do, when successful, is individuate evidential lines, like relics, license measurements and unify them by making them informationally relevant to each other, i.e. by making timecapsules. I also stressed the need to continue to pursue fleshing out stories to include additional detail. This is crucial because it constitutes a test. Insofar as additional details presuppose the story but are not guaranteed to yield information consistent with what is already known, by adding new relics to the timecapsule the story accrues confirmational boost because of the increase in mutual information, or it uncovers an anomaly that threatens to overthrow the story.

Lastly, this search for the form of coherence and how it is truth-conducive has uncovered an additional feature of justly scientific stories. This feature distinguishes justly scientific stories from just-so stories. *Prima facie*, just-so stories have empirical content, they purport to be about the world. Moreover, just-so stories appear to unify disparate phenomena. But what just-so stories do not do is place evidential lines within a relation of informational co-dependence from which we learn new things about the world and which can be further cross-checked by demanding that these new features, in turn, enter into further inferences.

# Chapter 4

## Back to the Beginning

### 4.1 Introduction

The title of this last chapter describes it in a few different ways. First, one of the upshots of the previous chapter's answer to the circularity worry associated with coherentist reasoning was that investigators should push to further refine stories of the past so as to include greater detail or to resolve events into their sub-events or to extend the temporal reach of the story. Doing so is essential, for doing so requires additional story-mediated measurements, which plays different aspects of the story off of each other, as well as off of other parts of background knowledge. The metaphor I developed was that of making stories do epistemic work in what I called epistemic leveraging. The case study of Ch.2 was an example of this, but there is much to the story of the origin of the Moon besides the giant impact conjecture. It is essential that geochemists can test the other aspects of the story. In §4.2, I will show how geochemists have been able to open a new window into the early solar system, pushing access further back in time toward the beginning, and in doing so have further tested the dynamics of accretion and have done something remarkable with the two isotopic chronometers discussed earlier.

In §4.3, I will pivot to an issue raised in the opening chapter but have not yet addressed, namely the sense that cosmology is in the midst of a methodological crisis. In Ch.1, I already

showed that the Cleland-Currie style approach to methodology doesn't work. But the way that it fails is related to the problems leading to the sense of crisis in cosmology. I'll argue that the sense of crisis is due to the way in which cosmologists conceive of predictions and the epistemic job it is supposed to, but cannot, do. I'll then show that my account of the story-mediated method is an attractive solution. The problem is that the old story of prediction and explanation that scientists profess does not do justice to the quality of evidence that can be produced, and so leads inevitably to the present sense of crisis where the testability of cosmology looks worrying. I'll show that my account sheds light on how the Standard Model of Cosmology has been tested so far.

Then, the dissertation will close, in §4.3.2, with a discussion of a proposal that has widespread acceptance amongst cosmologists, but which I worry poses a threat to cosmology. Driven by the conception of prediction I address in §4.3, cosmic inflation is postulated and thought to be confirmed by successful predictions that explain away otherwise puzzling features of the universe. But this supposed confirmation is merely a consistency check, and the evidential payoff of accepting inflation is that it cuts off access to what came before. Inflation, then, is self-undermining, for it precludes using astronomical observations to probe the highest energy scales that are experimentally inaccessible.

## **4.2 Probing the beginning of the solar system to test Accretion dynamics**

In Ch.3, I developed two metaphors that illuminate the inferential method employed by scientists investigating the past. The first was the idea that historical scientists are building timecapsules. The idea was that stories reconstructing past events individuate and unify many different relics. The chapter closed by developing the notion of epistemic leverage that sheds light on the nature of coherence that is essential to studying the past and which is meant to replace the worrying notion of "hanging together." In this section I will further employ and illustrate

these metaphors. One of the upshots of the previous chapter was that it is essential that scientists push to flesh out stories into greater detail or greater precision or to push their temporal scope. This is always an epistemic good, but for stories of the deep past it is essential given that scientists are often unable to replicate relevantly similar events or to make multiple different measurements of the same aspect. Given that stories invariably include many different elements, some of which are conjectural, and generally no measurement presupposes all of them, the only way to test all of the elements of a story is by making multiple story-mediated measurements. Although typically not measurements of the same parameter, multiple story-mediated measurements is essential and still tests the story by playing different subsets of the set of assumptions off of each other and off of background knowledge, as well as putting these different combinations into empirical contact with the world. When successful, doing so then yields additional information that further constrains the possibility space by fleshing out the story and ties different combinations of the elements of the story together such that new relics are added to the timecapsule.

Although in both Chapter 2 and below I focus on a few isotopic lines of evidence, these are just illustrations of the kind of investigative work that, together, make demands of our credence. Recall that the picture is that, in passing from the question of the impact origin to the fleshing out of the story to cover finer grained details is crucial for the epistemic standing, for doing so tests the inferential moves that came before. I've chosen to focus briefly on the radiochronometric lines here, but there are many other aspects of considerable importance and worthy of further philosophical investigation as well. Here are a few examples (with references to the scientific literature in the footnotes): 1) there is a substantial problem with the Moon's orbit following the giant impact.<sup>1</sup> 2) The lunar isotopic composition is anomalous insofar as giant impact simulations indicate the Moon forms predominantly the impactor, Theia, and yet the Moon is isotopically indistinguishable from the earth.<sup>2</sup>

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<sup>1</sup> For recent work on the evolution of the lunar orbit and the needed excitation, see Touma & Wisdom (1994, 1998); Ćuk & Stewart (2012); Ćuk et al. (2016); Ward et al. (2020).

<sup>2</sup> Simulation is crucial to the the overall investigation of the Earth-moon system's history. for a nice overview, see Melosh (2014). This anomaly arose with the earliest simulation work (see e.g. Cameron & Canup (1998);

There are many different examples of additional story-mediated measurements related to the early history of the solar system that would be suitable for illustrating the points above. I have elected to do so with recent work that extends our reach, so to speak, back in time further than thought possible. This work extends access to the very beginning of the planet forming process just a few thousand years after the start of the Sun, where the dominant dynamics in the accretion disk are very poorly constrained.<sup>3</sup> In so doing, this work illustrates the notion of epistemic leveraging by presupposing the poorly constrained accretion dynamics to yield new relics that provide new information concerning accretion.

This example case concerns the earliest history of the solar system, from the period just after the initiation of stellar fusion in our newborn Sun up to the formation of the bodies that were the precursors to the terrestrial planets. The story<sup>4</sup> that accounts for the formation of bodies after the start of the Sun is called “accretion.”<sup>5</sup> According to the story, the gas and dust surrounding a newly born star collapses into a planetary accretion disc. This collapse then initiates the first stage of accretion, where dust begins to coagulate. Fluffy dust balls form that, over time, grow to form bodies on the order of a Km in size. Very little is understood, at present, concerning this crucial epoch (more below). No satisfactory mechanism has been developed, but however it works, within about a million years the solar system was filled with thousands to millions of “mountain-sized” planetesimals. Once planetesimals have formed, the dominant dynamics is governed by gravitation and is complex, but far better understood. This

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Canup & Asphaug (2001)) and has persisted. Pahlevan & Stevenson (2007) proposed that the proto-Moon and Earth were in chemical equilibrium by way of large silicate atmosphere enveloping both. Some of the latest work has gotten even more exotic, and is supported by new simulations involving impacts beyond the corotation limit, where the problem may be solved by having a higher energy impact that lofts more of the Earth into orbit (see Lock et al. (2016); Lock & Stewart (2017); Lock et al. (2018); Ward et al. (2020)).

<sup>3</sup>This claim about accretion dynamics being very poorly constrained needs qualification. Many aspects of accretion are well understood in the sense that they are based on well-motivated assumptions and well-grounded theory, not to mention observations of many newborn stars. So, for example, the event that initiates planetary accretion, the collapse of the stellar nebula into a disk. Similarly, the later stages of accretion are well understood, though observational evidence in the form of observing new solar systems in these later stages is extremely limited, and so testing accretion by other means is essential. My comment about accretion being very poorly constrained applies mainly to the first stage, where very little is known and there is not much to go on for theorizing about the mechanism.

<sup>4</sup>Accretion is normally called a theory, but it is actually a story, in my sense developed in Ch.3.

<sup>5</sup>There is, of course, an analogous story of stellar accretion.



second phase is often dubbed “Oligarchic Growth,” for the dynamics ensure that larger bodies grow faster than smaller ones. What happens is that the largest planetesimals gravitationally sweep out and accrete most of the matter that resides within their “feeding zone,” that is, at their heliocentric distance. This stage is expected to end within about 10 million years, leaving the solar system with perhaps a few hundred planetesimals ranging in size from our Moon up to about the size of Mars. This stage ends with the dissipation of the remaining gas and dust in the accretion disk and so begins the final cataclysmic stage. During this phase, which is expected to last for on the order of another 100 million years, planets form by massive collisions with other oligarchs. As time passes, the frequency of collisions exponentially decreases, but the size of the collisions exponentially increases.<sup>6</sup>

As discussed earlier, the theory-mediated measurement technique that is relevant here is the use of the radioactive decay of isotopes of Uranium to isotopes of lead to measure the age of rocks.<sup>7</sup> This technique is a little more complicated than basic isotopic chronometry as one finds, say, when looking at how the Carbon-14 chronometer works. Most of the particulars of U-Pb dating are fascinating, but of no concern here.<sup>8</sup> What is crucial is that the relevant isotopes of Uranium decay to Lead with a known decay rate, and because the rate is slow enough, the parent Uranium isotopes have not gone extinct. This means that the original abundances of parent and daughter isotope can be estimated on the basis of appropriate samples. This, in turn means, that U-Pb chronometry is based on nomological relations that are well known, and needs to be calibrated by estimating the original parent isotopic abundances on the basis of the earliest known rocks in the solar system. Moreover, there are alternative long-lived chronometers that can be used as cross-checks, to make sure that the rocks being used as

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<sup>6</sup>I’ve presented here a fairly standard description of accretion. For excellent review articles, Zahnle et al. (2007); Condie (2015); Chambers (2004); Chambers & Wetherill (1998); Touma & Wisdom (1994); Morbidelli et al. (2012); Halliday (2014); Carlson et al. (2014). For discussion of the astrophysical environment within which the solar system formed, see e.g. Williams (2010); Chambers (2004); Adams (2010). For a non-technical overview see, e.g. Dalrymple (1994).

<sup>7</sup>In the interest of making this chapter reasonably self-standing, there is some overlap here with the description of the radiochronometry in Ch.2.

<sup>8</sup>For philosophical discussion of U-Pb chronometry, along with Hf-W chronometry (to be discussed below), see Fox (Under-Review); Bokulich (Forthcoming). For an accessible treatment of U-Pb chronometry, see Dalrymple (1994).

anchors for the initial abundances are, in fact, very ancient indeed.

It would seem, then, U-Pb chronometry is a story-independent means of measuring the age of planets and meteorites. In Ch.2 I showed how understanding the Earth's early history and, in particular, the origin of the Moon, a more sensitive chronometer is needed. The more sensitive chronometer used was a short-lived chronometer that uses the decay of Hafnium (Hf) to Tungsten (W). Hf-W is a far more sensitive chronometer, owing to its short half-life. Because Hafnium has a short half-life, it is now extinct and so the initial abundances cannot be measured or estimated. This is where story-mediated reasoning comes into play, for the giant impact story of the Moon's origin licenses coupling the short-lived chronometer to the long-lived one, which enables scientists to calibrate Hf-W. In the terms developed above, the giant impact hypothesis individuates new relics, the Hf/W isotopic ratios of the planets, Moon and meteorites, that provides a higher precision measurement of the timing of core formation and the the giant impact.<sup>9</sup> In the present case, the calibration goes in the other direction; short-lived chronometers are used to re-calibrate the long-lived chronometer's time zero.

Geochemists have continued to work on making the meteoritic timeline ever-more precise, by developing better mass spectrometric techniques as well as by doing further comparison analysis of timelines from various long-lived chronometers.<sup>10</sup> Remarkably steady in its improvement over the course of about 70 years, this work has been very fruitful labor, yielding much information concerning the taxonomy of bodies in the solar system and their heritages from parent bodies broken up long ago and so underpins and constrains the story of accretion. But by the 2010s, the timeline of the early solar system had been pushed to the limit of what the

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<sup>9</sup>In the final section of Ch.3, I mentioned that Myrvold (2017)'s work on Bayesian evidential unification is a potential way to formalize my conception of coherence. But, as noted, I have some worries about whether it is adequate for my purposes given that it seems to give confirmational boost to heuristic how-possible stories such as the luminiferous ether. This issue stands in need of further investigation, for Myrvold's notion of mutual informativeness lends itself to cases like this one. In the case of the Hf-W measurement of the timing of the giant impact, the U-Pb chronometer is made informationally relevant to the Hf-W chronometer by calibrating Hf-W's time zero. Then, in the present case, one could say that the story of accretion makes the short-lived Hf-W, Al-Mg and Mn-Cr chronometers informationally relevant to the U-Pb chronometer by licensing a re-calibration of the latter.

<sup>10</sup>This case study was gleaned from many papers. The most important, for my purposes, are Trinquier et al. (2008); Brennecka & Wadhwa (2012); Jacobsen et al. (2008); Bouvier & Wadhwa (2010); Kleine & Walker (2017).

long-lived chronometers can support and anomalies began to pile up, thus raising concern. The problems have largely been overcome now and the U-Pb timeline appears to be holding up very well. But how these anomalies were resolved is illustrative of the importance of continuing to flesh a story out by pursuing additional story-mediated measurements.

The likely problem was with the relics taken to bear the mark of the initial isotopic abundances. In the geochemistry literature, such relics are called “anchors,” for they fix the time zero for the clock. Two anchors are needed, one for the parent primordial isotopic abundance and one for the daughter. These relics are meteorites selected as representing the oldest and, therefore, most primordial samples of the protoplanetary disk. The class of meteorites used for the U anchor is the “angrites,” which are now known to be quite an interesting class. They derive from the mantle of the first generation, so to speak, of differentiated bodies, and are now known to be formed by volcanism. What makes them good U anchors is that they formed very early, though not as early as the Pb anchors (see below), and that they have extremely low Pb content, aside from the radiogenic lead produced as U-238 and U-235 decayed. For primordial lead, the anchors used are so-called CAIs (calcium aluminum rich inclusions). CAI’s are small inclusions found within an ancient class of meteorites called “chondrites.” Chondrites formed very early, during the first phase of accretion and are regarded as representing samples of the bulk solar system. Though undifferentiated, chondrites do appear to have undergone significant thermal processing since their formation. But the CAI’s they contain, are much less variable, and due to their thermal properties, are more durable and thought to be among the oldest solids that condensed out of the slowly cooling gas in the accretion disk. CAI’s also contain very little uranium, and so are well-suited for representing primordial lead in the solar system, as the lead contents have not been significantly modified by uranium decay.

This worked pretty well for some time, but as mass spectrometric techniques used for measuring the isotopic ratios of samples to be dated using the chronometer were refined, and as more types of meteorites were analyzed, concern started to mount as anomalies began to pile up. An obvious place of concern is the primordial isotope ratios that set time zero. When work-

ing at a coarser grain of precision, the initial primordial parent and daughter isotope ratios are not so crucial. But with increasing precision, uncertainty in the initial abundances becomes a significant limitation. There are two parts to the problem. First, being a long-lived chronometer, U-Pb has low sensitivity on the time-scale relevant to the formation of the anchors. And so the early dynamics of accretion potentially introduce errors that, given the push to higher precision, have a large effect even though arising from dynamics below the level of sensitivity of the U-Pb systematics. The second issue is similar, but concerns small changes to the anchors by thermal processing that occurred later in their history, but again are undetectable given sensitivity limited by long half life. Recall that subsequent thermal processing can alter the U-Pb chronometric determination, for what isotopic chronometers track is the time since parent and daughter isotopes were last fractionated. Fractionation can occur by crystallization if parent and daughter are differentially compatible with minerals as they form. Also, since Pb is volatile, heating can result in Pb loss and so reset the clock. Perhaps, then, the anchor meteorites are not pristine samples of primordial abundances but also reflect complex histories.

To investigate this, a more sensitive chronometer is needed, and so geochemists turned to Hf-W, the systematics of which have been worked out in large measure through the work on the giant impact, as well as the short-lived Al-Mg and Mn-Cr chronometers. In very similar fashion to the case study in Ch.2, story-mediated reasoning plays a crucial role, though this involves a different set of assumptions. Here the accretion story, as opposed to the giant impact conjecture, provides the necessary causal link that licenses coupling the short to the long-lived chronometer. Such chronometers are far more sensitive on short time scales within the window of time when the parent isotope is extant, they are ideal for investigating whether the classes of anchor meteorites are too coarsely defined, as should be further broken down to reflect differences in the environment in which they formed or to reflect more complicated histories. What this work has shown is that the angrites do, in fact, have more complicated histories and so careful selection of which particular angrite is used as the anchor is required and that the primordial uranium isotopic ratio inferred requires corrections that take into account

the subsequent thermal processing that the short-lived chronometers reveal. As for the CAIs used to anchor the primordial lead ratio, the story-mediated boost in sensitivity reveals that CAI's, too, are a diverse class that formed with material with variable lead ratio and licenses corrections to the primordial lead ratio. With these corrected values for the anchoring ratios, the U-Pb timeline has been re-calibrated and the anomalies that had accumulated have mostly been resolved.

Resolving anomalies is important and marks an important success, but this example has one final aspect of import for me. In both cases, this work successfully tests the story of accretion, for it was able to individuate and then tie together new relics. Measurements presupposing the story of accretion, moreover, yield new information that both shows that the story of accretion needs to be further complicated, and provides new empirical evidence constraining how the story needs to be complicated. In doing so, geochemists have epistemically leveraged their way further back in time, into the first phase of accretion, when the CAIs and angrites formed. The angrites show that complex geochemical processing was occurring on the angrite parent body at a very early time. The CAIs now can be used to probe back into the first few thousand(!) years after the start of the Sun, revealing new details concerning the state of the accretion disk when it collapsed and how it was heterogeneously distributed. Both are important, for, recall, the first phase of accretion (from fluffy dust balls to bodies just large enough to be self-gravitating) is the one that is so unknown and poorly constrained.

Although this pattern of reasoning has the appearance of circularity, it is an evidentially productive form of coherence. The admonition of this method is to propose conjectures where necessary, and test them by making them do epistemic work to yield new evidence concerning finer-grained details. Story-mediated reasoning thus enables scientists to garner more evidence and gain access further back to the beginning.

### 4.3 Crisis in Cosmology?

In this final section, I will pivot to some pressing issues with the attempt to push our epistemic reach back to the very beginning of it all, i.e. cosmology. The preceding discussion added the final piece needed in order to shed light on what I, and others in the high energy physics community, take to be a brewing methodological crisis in cosmology. The main concern here I'll call a crisis of predictability. The underlying problem, I argue, is that high energy physicists tend to think of their aim as providing an explanation of observable phenomena<sup>11</sup> in terms of some underlying dynamical reality. Confirmation (or disconfirmation) by way of successful (or failed) prediction is then taken to be the highest arbiter of truth for dynamical explanations. But this results in a cycle of setting the epistemic bar too high and then too low. By setting the epistemic bar too high, scientists are unable to get confirming predictions and so are forced to lean on non-empirical considerations—with explanatory power, mathematical beauty and parsimony, perhaps chief among them—as guides to truth. The bar is then set too low, for non-empirical “virtues” are heuristic guides to theory development and are not, themselves, truth conducive.<sup>12</sup>

In cosmology and other aspects of high energy physics, the emphasis on prediction initially sets the bar too high because the novel predictions made by theories that go beyond the standard model of particle physics concern goings-on in a dynamical regime that is inaccessible by experimental means. Although the Large Hadron Collider is impressively powerful, it is incapable—by many orders of magnitude—of probing the energy scales needed in order to corroborate predictions made by various forms of string theory or quantum gravity, etc., and which are necessary given that the standard model of particle physics must break down at these scales.<sup>13</sup> This leads to a sense of crisis, for the motivation for reconstructing the universe's his-

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<sup>11</sup>“Observable” here is meant to be broadly construed, not meant in the narrow sense of what can be observed without aid.

<sup>12</sup>For physicists discussing these kinds of troubles, see, e.g. Hossenfelder (2018); Unger & Smolin (2015); Dawid (2013). For philosophical discussions, see, e.g. Smeenk (2014); Earman & Mosterin (1999).

<sup>13</sup>Not to mention the fact that General Relativity and Quantum Mechanics are known to be mutually incompatible.

tory, as Zeldovich nicely phrased it, is to use cosmology as a “poor man’s accelerator,” in the hopes of probing the otherwise inaccessible highest energy scales for empirical constraints on fundamental physics. But if our only guide to the truth of these eventual theories is successful prediction, there will be no way to confirm them. And so the situation looks hopeless.

This clearly sets the bar too high, so something more modest is needed. Indeed, while confirmed novel predictions can be spectacular, they are actually fairly rare in science. Insofar as successful theories explain the phenomena that they are about, explanatory power seems like a reasonable indicator of success. Moreover, prediction and explanation are closely related, especially so by Hempel (1942)’s influential deductive nomological model of explanation—where to explain a phenomenon is to be in a position to derive it as a prediction from some theory. So, while more modest, the idea that cosmological theory (or string theory or other contenders in high energy physics) is testable and confirmable on the basis of prediction seems to save the day. Cosmology is predictive in the sense that it either predicts that the state of the universe now is as we observe it to be, or it is falsified. Indeed, this is just what Dawid (2013) has proposed. In his defense of string theory and cosmology, he points out that these theories aren’t predictive in the way one would hope. He then recognizes that historical sciences like paleontology have this very same kind of problem. Nevertheless, he observes, paleontologists make progress and put theories to the test, and so he proposes that cosmology and string theory adopt the kind of methodology he sees as operative in historical sciences. And this methodology centers on non-empirical confirmation.<sup>14</sup>

Back in Ch.1, I discussed Cleland’s insight that there is something worrying about the notion of prediction in the historical scientific context. Cleland rightly points out that an important part scientific confirmation is that false positives—where the prediction is corroborated by observation, but only because of an accident and having nothing to do with the putative truth of

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<sup>14</sup>Dawid’s account of non-empirical confirmation does not only feature explanatory power. For Dawid, a crucial element of non-empirical confirmation is mathematical elegance and the pursuit of the same “theoretical program” that has been successful in the past. A view like this is especially appealing for string theorists, for mathematical beauty is an important part of the motivation for string theory, and Dawid also thinks that string theory is actually a direct continuation of the very same theoretical approach that led to the empirical success of the standard model of particle physics.

the theory being tested—must be ruled out. But this cannot be done owing to the uniqueness of the universe and so it seems we can neither confirm nor disconfirm cosmological hypotheses. If a hypothesis concerning the past predicts what is observed today, then it has passed a consistency test. That such a hypothesis is consistent with the way the world is later is crucial, but this only indicates that it is possibly true. So, if the epistemological picture is one in which prediction is the highest the test, then a story's consistency with what we already know has passed an important test and accrued confirmational boost. But this is too low of a bar, and victory is declared too soon.

Moreover, non-empirical virtues are insufficient to do the needed extra epistemic work. Here, again, Cleland has important insight. Although I ultimately rejected her proposed solution, Cleland noted that non-empirical virtues such as explanatory power as well as her preferred virtue, viz. common cause reasoning, stand in need of empirical grounding. Without such grounding, any appeal to them as truth-conducive requires substantive appeals to metaphysics.<sup>15</sup> I gather that resting their laurels, so to speak, on substantive metaphysical pre-suppositions is not appealing for most scientists, and so Dawid's proposal does not look very attractive. Taking a possibly true explanation as a confirmable prediction makes confirmation too easy, and so the predictability crisis looks again hopeless.

These kinds of predictability concerns lead Unger & Smolin (2015) to consider other ways in which cosmologists need to amend their approach. Smolin thinks that historical laws, which explain how the laws of physics that govern the universe have change over time, are needed.<sup>16</sup> The idea seems to be that when physicists study or describe the law-like evolution of systems, they do so from the perspective of an observer outside of the system. But the laws governing such a system's behavior are insufficient to characterize its evolution insofar as shifts in dynamical regime take place. This is much as I have argued earlier, with respect to the nature

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<sup>15</sup>Recall that Cleland sought to provide the needed empirical grounding for the principle of the common cause with what she takes to be an empirical claim, viz. the overdetermination of the past by the present. I argued, in Ch.1, that the overdetermination thesis ultimately turns out to be a substantive metaphysical claim of just the sort that is deeply worrying and so undermines the claim that common cause reasoning is truth conducive.

<sup>16</sup>Smolin also thinks that a new conception of time is going to be needed as well.



of stories. But because the universe is causally closed—it contains the totality of all that we can causally interact with—it seems that historical laws that govern the universe as a whole are needed in order to really understand how the universe came to be and how the local laws were set as they are. Such historical laws, then, concern degrees of freedom had by the universe over and above the degrees of freedom had by the subsystems within.<sup>17</sup>

My worry concerning this kind of approach is simply that it will lead to more of the same problems. Such historical laws will make claims regarding the universe as a whole that—in principle, because we cannot step outside of the universe—cannot be checked. The burden will then be shifted to such laws’ ability to predict the laws of local physics that we already know, and so will come down to their explanatory power. But this is insufficient to establish any more than their consistency with what we already know. Again, I gather that Unger & Smolin’s hope is more, in the end, than to articulate how-possible stories. Note, however, that I am not here claiming that there cannot be historical laws or that this kind of approach should not be considered. Indeed, I think theoreticians should be free to explore as wide a range of possibilities as they can envision. My worry is only that, supposing some interesting headway is made, there must come a time when empirical evidence is brought to bear on them. And so this kind of approach will not avoid the predictability crisis.

The situation, however, is not hopeless. My account of story-mediated reasoning and epistemic leverage provide an attractive alternative conception of how the cosmological story might be tested. In the next section (4.3.1, I will briefly explore a couple of ways in which my account illuminates the important ways in which the so-called “ $\Lambda$ CDM” model of cosmology has been put to empirical test. My account does justice to the probative value of the evidence that has been found, even as the standard way of thinking in terms of prediction fails. I will then close with §4.3.2 a discussion of a different methodological issue that I find worrying. Although my account of how stories can be tested casts positive light on much of cosmology, one subarea,

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<sup>17</sup> Another who thinks that what it means for cosmology to be properly understood as a historical science is to conceive of it as the study of the universe as a whole, which is more than the study of the structures within it, is Pearce (2017).

viz. cosmic inflation, looks worrying. The issue here is that, although I've now argued that historical scientific inquiry is not much different from non-historical, there are a few matters that might need special consideration, and, I fear, inflation runs afoul of them and, in doing so, pulls the plug, so to speak, on the poor man's accelerator and so undermines the very motivation for doing cosmology.

### 4.3.1 Cosmology qua story science

In this section I will briefly sketch my answer to this predictability crisis in cosmology. I will start by showing that the standard way in which some recent work on the CMB is construed—as confirmed predictions of the so-called  $\Lambda$ CDM model—is highly problematic. Instead, they should be regarded as story-mediated measurements and so are successful instances of epistemic leverage. The cosmological story has been made to do epistemic work, which reveals new dynamical details relevant to the evolution of the universe, despite it making astonishing simplifying assumptions. As with the giant impact and accretion cases discussed earlier, my account is not radical in the sense that it actually upholds much of the epistemic standing scientists hope for. Prediction simply cannot bear the epistemic weight that scientists place on it, whereas story-mediated measurement can. Moreover, although story-mediated measurement enables investigation of unique and complex particulars that cannot be intervened upon, it is not much different from the way in which theories are put to use in nomothetic, experimental work. I regard my account, then, as simply a better explication of the inferential method that scientists employ. It is, however, also a normative account, in that it makes a few demands. Unless the elements of a story can be made to do epistemic work by individuating additional relics or licensing additional measurements that flesh the story out in more detail or make it more precise, such elements should not be regarded as anything more than how-possible additions that stand in need of evidential support. I will now focus on how cosmologists have

managed to fulfill this epistemic duty that my account requires.<sup>18</sup>

At this late stage in the dissertation, I'm not going to go into much detail concerning a very different (not to mention theoretically complicated) kind of case so different from the those considered earlier. A brief sketch will suffice to show at least the promise and attractiveness of my account for the field of cosmology, as well as show that it is not simply a philosophical explication of radioisotopic chronometry. I will restrict this discussion of cosmology, then, to just a few of some of the very recent results in cosmology concerning the CMB due to the Planck satellite mission and how they should be regarded according to my account.<sup>19</sup>

The first thing to note is that the  $\Lambda$ CDM model (sometimes also called the Standard Model of Cosmology as well as the Concordance Model) is actually a story precisely in the sense in which I have defined stories. It very much has a narrative structure to it, in the sense that it consists of, among other things, a sequence of different dynamical regimes with associated turning points that link them. I will, therefore, refer to it variously as the  $\Lambda$ CDM story or simply as  $\Lambda$ CDM. Moreover, many of the assumptions required to link the different dynamical regimes are highly conjectural or startling simplifications. Part of this is due to the intractability of General Relativity (GR), which is one of the central theories forming the backbone of the story. There are only a handful of toy models for which GR has a known analytical solution, and so the cosmological story must presuppose a simplifying toy model. The model of GR that is the backbone of  $\Lambda$ CDM is the so-called FLRW model. One of the presuppositions required by the use of the FLRW model is that the universe, on the largest scales is homogeneous and isotropic, meaning that the matter and energy contents of the universe are uniformly distributed such that the universe looks the same no matter which direction one looks. The model then

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<sup>18</sup>I'll then close this work out with §4.3.2, by further demonstrating my account's normative punch, showing that illuminates some new and deep worries regarding cosmic inflation.

<sup>19</sup>Note that in the following discussion of cosmology, I am going quickly through very technical terrain. I omit most technical details, as my argument and aim is not technical in nature in here. I am essentially going through and rehearsing the standard standard line on some broad conceptual points and then showing how they ought to be slightly re-conceived in light of my framework. Where I make critical remarks, I included citations to show, in part, that what I am calling the "standard view" really is that. For more technical discussions of cosmology, I refer the reader to a standard textbook treatment such as Ryden (2017). For references to further philosophical issues in cosmology, including to more technical philosophical work, see ?.

treats the matter and energy as perfect fluids governed by equations of state that determine how each constituent fluid's pressure varies with density. That such highly idealizing assumptions are required is not surprising, given the complexity of GR, but what is surprising is how well this modeling assumption has turned out to work. It seems that the universe really is well approximated by treating it as a system of perfect fluids, despite the fact that this is not at all what one would expect given the nature of matter as well as observations of a not at all homogeneous cosmos. The reason the FLRW model works so well, it is thought, is that the scale relevant to cosmology is vastly larger than the scales astronomers observe. I will postpone discussion of the status of this assumption (that the universe is approximately FLRW) for a little later.

One crucial thing that follows directly from assuming the FLRW model concerns the universe's expansion history. In particular, given that the universe is observed today to be expanding, it follows that the universe has always been expanding.<sup>20</sup> So, going back in time, the universe always contracts until it eventually is so dense that the spacetime breaks down as the curvature goes to infinity. Tracing the history back beyond this time, if possible, will presumably require a quantum theory of gravity that solves the problem of singularities in GR. Moreover, this expansion history, coupled with knowledge of thermodynamics, enables an approximate thermal history of the universe to be reconstructed. In the terminology developed in §3.3.1, the FLRW model plus background knowledge including astronomical observations establishes a story-line to be further refined into a sequence of dynamical trajectories linked by turning points. Going back in time, as the universe contracts, it is heated, such that, at different times, different aspects of the standard model of particle physics become the dominant dynamical regime with phase changes marking various turning points. The narrative character of  $\Lambda$ CDM is therefore essential.<sup>21</sup>

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<sup>20</sup>Note, though, that the rate of expansion can vary according to the model. Indeed, there have been periods of accelerated and decelerated expansion.

<sup>21</sup>In addition to the authors discussed in the preceding chapters who, like Cleland, are suspicious of the narrative character of theories of the past, Goenner (2010); Grignon (2012) have expressed similar worries with respect to cosmology. Goenner thinks that the story-telling aspect of cosmology is a symptom of physicists playing "make believe." Grignon argues that the narrative way of knowing in cosmology is in tension with the nomothetic way

Some of these early universe regimes and turning points can be investigated with the tools of the standard model of particle physics, but eventually the energy and density scales of the universe far exceed where current high energy physics is applicable. As powerful as the Large Hadron Collider is, the energy scales involved in the very early universe were many orders of magnitude higher than the scales at which the standard model of particle physics breaks down. In the early universe, then, two of our best theories in physics break down. These are problems, to be sure, but they are also precisely what motivates the study of early universe cosmology. As I mentioned above, at least for the fundamental physics community,<sup>22</sup> the promise, or at least hope, of cosmology is that the early universe can be used as a poor man's accelerator, probing energy scales many orders of magnitude higher than what can be probed experimentally.<sup>23</sup> I will use the term 'primordial' to refer to these early universe regimes and states that are otherwise inaccessible. Cosmology is a context in which existing theories can be tested in new ways in the effort to probe new physics. Moreover, even before making much progress on with respect to probing higher energy scales, cosmology has already had much success as a probe of new physics, for cosmology and astrophysics have discovered that the sorts of matter and energy that are experimentally known comprise only about 5% of the matter and energy in the universe. Even though not much is yet known about this mysterious "dark sector" that comprises 95% of the universe, the discovery of it is not something that could have been done without astrophysics and cosmology. And so the high energy physics community is clearly wise to look to the heavens and to the past for help.

Recently, a massive international and interdisciplinary effort to characterize what is arguably the most important relic from the deep past, viz. the CMB, culminated in the European Space Agency's Planck mission, which released their final data and findings in 2018. The Planck findings are vast, but I will restrict this discussion to one important piece. Since the ac-

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of knowing in physics. I find both views implausible.

<sup>22</sup>Here I have in mind those working on theories in high energy physics beyond the standard model, as well as those working on successor theories to GR.

<sup>23</sup>A particle accelerator capable of probing these energy scales would not fit in the solar system, and would require more energy than the Sun generates (Lacki (2015)).

cidental discovery of the CMB in 1965, it has been a central focus of cosmology. The CMB fills all of space with a nearly uniform radiation at a temperature of  $2.7K$ . According to  $\Lambda$ CDM, by about one second after the Big Bang, the universe had cooled sufficiently that nucleons formed and the event known as Big Bang Nucleosynthesis occurred, where protons and neutrons fused to form mainly helium, some deuterium and a little bit of lithium. The temperature was too high, however, for stable atoms to form as electrons were immediately ionized by the primordial photons that dominated. Big Bang Nucleosynthesis seeded the universe with the nuclei that would eventually form the gas that would gravitationally collapse and give rise to galactic nebulae and eventually the first generations of stars. But after nucleosynthesis, the temperature and density of radiation was very high and so photons and electrons were highly coupled in the form a dense plasma; photons could not freely stream for their mean free path was tiny and so photons were unable to travel far before being scattered off of electrons. By about 380,000 years later, the universe had expanded sufficiently for the temperature and density to drop to the point where electrons could be bound by protons into neutral atoms and the photons could then freely stream, with their mean free path suddenly changing from very small to cosmological scale. This event, called “Recombination” for it was triggered by electrons recombining with protons to form atoms, ended with the sudden release of the CMB. Observationally, the CMB appears as light emitted from a surface (the “surface of last scattering”) that is continually receding from us (due to the expansion of the universe), depicted in Fig.4.1.

The CMB is remarkably thermally uniform, but there must be small temperature variations in it that reflect density variations because without such density perturbations no large structures will form later. The issue here is that structure formation required variations in gravitational potential, where slightly overdense regions seed galaxy development while underdense regions seed voids. And so the observational program for cosmology has largely focused on characterizing CMB anisotropies, for they are marks preserved from the primordial states that preceded the dynamical regimes that are well understood by existing theories.

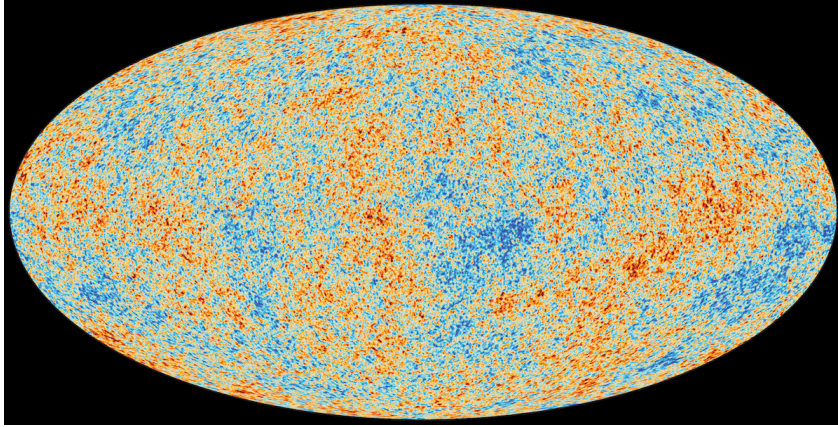


Figure 4.1: This image is the map of the temperature variations in the CMB, showing its remarkable uniformity. The average temperature is 2.7K, the difference in temperature between the hottest spots (dark red) and coldest spots (dark blue) is about 0.00003K. The temperature differences coincide with tiny density variations in the matter distribution of the Universe at the time the CMB was emitted. *Credit: Planck 2018 [Aghanim et al. (2020)]*

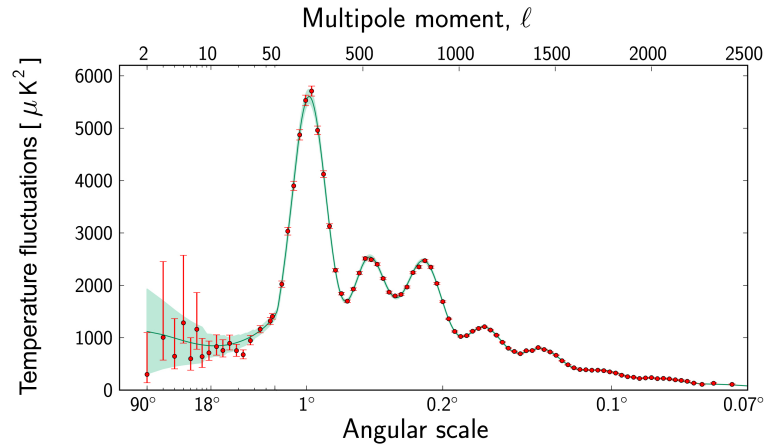


Figure 4.2: This plot is the CMB Power Spectrum. It shows the relation between angular scale (distance between regions on the surface of last scattering from which the photons were emitted) and intensity of the temperature fluctuations observed. The plot shows the intensity of the average temperature difference (y-axis) between regions at various angular separations (x-axis). The red dots are the latest Planck satellite measurements (with associated error bars). The green curve is the predicted power spectrum derived from the  $\Lambda$ CDM model. See text for discussion. *Credit: Planck 2018 [Aghanim et al. (2020)]*

The work on CMB anisotropies has culminated with the Planck mission, which has generated the highest precision quantitative study of the CMB yet and yielded enormous amounts of information about the universe at recombination, but also about both the present day as well as the primordial universe (Aghanim et al. (2020)). We now know that the CMB is very close, but not quite, uniform. The CMB is uniform to within 1 part in  $10^5$ ; the temperature variation between the hottest and coldest regions a mere  $0.00003K$ . Fig.4.2 depicts the most important Planck result, for my purposes here. The power spectrum plots the measured intensity of the temperature variations (y-axis) between regions of the CMB at different angular scales. What is astonishing about this plot is that it shows that the apparently randomly distributed anisotropies are nevertheless highly correlated. The power spectrum depicted here is quite literally the acoustic profile of the universe on scales far larger than can be observed otherwise,



with the large peak indicating the fundamental frequency, and the smaller peaks corresponding to the universe's harmonic overtones. The interpretation given by  $\Lambda$ CDM is that temperature variations are correlated with density variations at the time of recombination. Before recombination, the gravitational potential in overdense regions was larger, and so gravity acted to compress the plasma. But radiation exerts pressure outward, and so the gravitational compression causes pressure to build, leading to rarefaction. So the plasma was subject to an acoustic oscillation that is recorded by the CMB, not in the form of sound waves but in the form temperature fluctuations, because compressed regions were heated while rarefied regions cooled. Finally, at recombination, the temperature fluctuations were “frozen in,” in the sense that the CMB photons were, thereafter, decoupled from matter. Even though the acoustic oscillations then stopped, the photons bear the mark of the state of rarefaction or compression as a slight decrease or increase in their energy compared to the average.

The large peak in the power spectrum corresponds to those regions on the surface of last scattering that, at recombination, had experienced one complete compression.<sup>24</sup> These regions correspond to the largest density perturbations. The peak's horizontal location is most sensitive to the spatial curvature of the universe. The energy density determines the curvature, and the energy density contributed by matter and radiation is far below the critical density, which is the density that makes the curvature of the universe zero, i.e. flat. But the location of the first peak indicates that the universe is very close to flat (more on this in §4.3.2), so the first peak is set by the dominant source of energy, so-called dark energy.

The second peak corresponds to regions with smaller density variance such that they oscillated a little faster, one complete compression and, at recombination, were at maximum rarefaction. This peak's relative height is determined by the ratio of photon to baryon (normal matter) density. Moreover, the third peak corresponds to regions that oscillated faster still and were at maximum compression again at recombination. This peak's height relative to the second is a function of the ratio of dark matter to baryonic matter.

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<sup>24</sup>For an accessible discussion of power spectrum dynamics, along with helpful visualizations, see Wayne Hu's lecture notes available online at <http://background.uchicago.edu/~whu/intermediate/map5.html>.

The red dots in Fig.4.2 and associated error bars are the values measured by the Planck satellite. The green curve is standardly construed by cosmologists as the power spectrum predicted by  $\Lambda$ CDM. Indeed, the the observed power at different angular scales is in astonishingly good agreement with the green curve, and so it is not surprising that this would be regarded as a confirmed prediction. Moreover, given the difficulties of the epistemic situation inherent in studying a unique target system that cannot be intervened upon, etc., verifiable predictions are certainly hard to come by. So it is not surprising that this would be celebrated.

The problem, however, is that the green curve is not a prediction, it is a curve fit.  $\Lambda$ CDM cannot predict a power spectrum curve without being first provided with values for several key parameters. And the Planck power spectrum data, combined with other observations, tightly bounds the values of the parameters related to the energy density of dark energy, the density ratios of dark matter, baryonic matter and radiation, as well as the Hubble parameter (a measure of the rate of expansion). The green curve is then generated by plugging these values into  $\Lambda$ CDM.

It is precisely here where my account of story-mediated measurements sheds light. Instead of construing scientific knowledge in terms of predictive or explanatory power, it focuses on the epistemic role of licensing measurements that add greater precision and additional detail and are capable of revealing anomalies that indicate inadequacies. The power spectrum is a crucial part of a series of story-mediated measures of features of the universe that either cannot be known with such great precision or are otherwise unknowable. The key is the recognition that  $\Lambda$ CDM plays a crucial role in turning the power spectrum data into measures of cosmological parameters with physical significance. Without presupposing  $\Lambda$ CDM, there is no sense in which the first peak of the data is due to dark energy, or that the other peaks concern matter and dark matter. So on my account, the Planck result really does constitute an important empirical test of  $\Lambda$ CDM, for there was no guarantee that these higher precision story-mediated measurements would be compatible with the previous alternative measures of the relevant parameters.

It is important to note that the alternative measures of cosmological parameters are also

story-mediated, though to different degrees. Perhaps the most story-independent measure of one of the parameters, is the “distance ladder” measure of the Hubble parameter. I will not discuss the details of this here, but would like to stress that there is currently a tension between the value measured by Planck and that measured using the distance ladder technique. This anomaly is currently the subject of intense interest. From my perspective, this tension further demonstrates what I argued for in the previous chapter. There I argued that presupposing a story to make the measurements that are taken as tests thereof does not pose a vicious circularity problem so long as the resulting measurements are not guaranteed to be consistent with the evidence so far. Moreover, I also argued that the discovery of anomalies are not to be immediately taken as disconfirming evidence. The Hubble anomaly is an example of an inconsistency, but even as such it is a discovery made possible by  $\Lambda$ CDM, which may turn out to be the discovery of more new physics.<sup>25</sup> Anomalies, then, are a good sign, provided that not too many of them pile up, so to speak, for they are seeds of further discovery of finer-grained causal details that have not yet been accounted for that have been individuated by the story. So they are further sign that the story is epistemically earning its keep. In this regard,  $\Lambda$ CDM has proved to be a remarkably useful epistemic tool, licensing the discovery of new features of the universe, not just in the past, but in the present, that are otherwise inaccessible. There are still challenges and open questions, to be sure. The CMB is mostly exhausted now as an observationally window, and so new windows need to be opened. Moreover, the predictability crisis need not pose such a threat. Prediction and explanatory power cannot take cosmology as far as they would like, but this is not because of problems inherent to cosmology’s untestability or anything like that. Story-mediated inferences demonstrate  $\Lambda$ CDM’s probative value so far and demanding that it continue to underwrite new measurements promises to lead to further

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<sup>25</sup>Interestingly, the sides in the debate concerning which measure of the Hubble parameter is correct have fallen recently into a debate over prediction. The defenders of the distance ladder measure of the Hubble parameter use the fact that the early universe cosmology community construes the Planck results as predictions for their advantage. According to the distance ladder defenders, the Planck estimate is a failed prediction, disconfirmed by the distance ladder method, which is not a prediction but a true measurement. See, e.g. Knox & Millea (2020); Kenworthy et al. (2019). Needless to say, I disagree with this assessment, for the distance ladder folks are attempting to declare defeat for  $\Lambda$ CDM too soon.

refinements or to the discovery that an altogether new cosmological story is needed. And this is all we are entitled to ask of a either a story or a theory.

### 4.3.2 The End of the Beginning:

#### **Or, does inflation pull the plug on the poor man's accelerator?**

There is a sense in which calling the Planck power spectrum results a prediction is perhaps not such a big deal. In this section, I will show that the problematic conception of prediction does lead to something deeply troubling regarding inflation. I'll start with a very brief description of the problems that inflation is postulated to solve. Inflation solves these problems by postulating a new dynamical regime that explains those problems away. It is considered by many to make successful predictions, moreover, but these predictions are merely consistency checks. And so it is explanatory power that is doing the work of confirmation, and this is problematic. I'll then return to an issue raised in §3.3.2, which sheds light on an aspect of cosmology's being a historical science that no one has yet considered, namely that its success is, in part, driven by the contingency of history. But inflation is then cast in worse light, for inflation's putative success consists in erasing that contingency and so undermines the possibility of probing back to the beginning. This is deeply worrying insofar as the motivation for doing cosmology is to use it as a poor man's accelerator to probe, if Nature cooperates, the Planck regime.

The problems that inflation solves are the so-called flatness and horizon problems, which, along with several other issues are often referred to as fine-tuning problems. One puzzling feature of the universe is that astrophysical observations indicate that the universe is close to having zero spatial curvature, meaning that it is close to flat. The curvature is determined by the total energy density of the universe. In  $\Lambda$ CDM, the density is parameterized as  $\Omega$ —the ratio of the energy density divided by the critical density, which is the density that makes the curvature zero. So one of the story-mediated measurements related to the Planck power spectrum mentioned above is that  $\Omega = 1$  to within about 1%. The puzzling thing about this, however, is that the dynamics of gravity and expansion continually drive the  $\Omega$  away from 1.

Spatial flatness for the universe is an unstable state, such that if so close to flat now, it had to be even closer flatter going back in time. Indeed, in order for the universe to have  $\Omega$  within 1% of 1, now, at the time of recombination  $\Omega$  differed from 1 by no more than 1 part in  $10^5$ . Going further back, at the time of nucleosynthesis  $\Omega$  differed from 1 by no more than 1 part in  $10^{15}$ . And the initial condition of the universe in the Planck regime was  $\Omega = 1$  to within 1 part in  $10^{60}$ ! This is a troubling finding, for there is no reason to expect the universe to have been close to flat initially, let alone so incredibly close. Moreover, it seems that in a universe that is not this close to flat, galaxies will either not form because the expansion rate is too fast and the matter density necessary dilutes too quickly, or the universe will gravitationally re-collapse in a Big Crunch long before life-supporting conditions can arise. So it seems that the particular initial condition was finely tuned in order to yield a universe like ours.

The horizon problem is really a problem related to the notion of causality. The issue here is that the CMB is so thermally uniform and even the temperature fluctuations across the sky are highly correlated. The reason this is puzzling, indeed, deeply puzzling, is that this uniformity is thermal equilibrium. Equilibrium is the result of causal interactions, but the regions of the CMB that are separated by as little as 1 deg on the sky are space-like separated, meaning that they are not in causal contact now. The presently observed surface of last scattering is composed on a great many regions that are now causally isolated. Moreover, since the expansion history known, these region's past light cones can be constructed, and looking back in time shows that not only are they causally isolated now, but they always have been. This is extremely puzzling, for the initial condition of the universe had to be very strange indeed. It seems that the initial condition was had a strange kind of pre-established non-causal harmony of correlations between space-like separated regions.

The horizon problem, coupled with the flatness problem, seem to be crying out for an explanation. That  $\Lambda$ CDM points toward such a finely tuned initial condition is extremely inelegant, for the primordial state was far more improbable than walking up to a park bench in the middle of a hurricane and finding a pencil standing on its point. Combine this improbability with the

fact that had the initial condition been even slightly different, we would not be here to puzzle over it, and the fine tuning problem seems to have real teeth.

Inflation comes to the rescue by providing an elegant explanation of these features of the universe.<sup>26</sup> Inflation postulates a new high energy quantum field—the inflaton field—that dominated during a new dynamical epoch just before Big Bang Nucleosynthesis. The inflaton field exponentially accelerates the universe’s expansion for a very brief time, and from this postulated expansion predictions can be derived. Inflation predicts that the primordial energy density present in the universe is rapidly diluted to zero. Meanwhile, the inflaton field has a constant energy density, so the total energy density of the universe becomes completely dominated by the inflaton energy. Moreover, the scale factor of the universe rapidly expands, such that  $\Omega$  is driven to 1 within 1 part in about  $10^{60}$ . This solves the flatness problem because the primordial  $\Omega$  need not be close to 1 at all, for that initial energy density is diluted away by the expansion, and replaced by inflaton energy that the expansion drives to  $\Omega = 1$ .

The horizon problem is also elegantly solved. For inflation predicts that regions very near each other and so in causal contact in the primordial state are rapidly driven to space-like separation. The postulated exponentially accelerated expansion makes all of the light cones of the regions on the surface of last scattering overlap in the past, and so no spooky space-like separated correlations are needed to explain what we observe today. Moreover, even though the other predictions that inflation makes, such as regarding CMB polarization, etc., have been observed, inflation is highly predictive and, indeed, these predictions have been confirmed. This is the standard line that cosmologists give, and is the basis for widespread agreement that inflation is more than a how-possible explanation added to  $\Lambda$ CDM, for it is evidentially well supported.

I now want to turn to what I see as deeply worrying about inflation. First, the standard claim that it is predictive is problematic for the reasons I have argued in the previous section. At most, inflation predicts what is independently known about the later universe, and so these predictions

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<sup>26</sup>See Guth (1981); Guth & Steinhardt (1984); Starobinsky (1982) for the seminal work proposing inflation. For recent criticisms from scientists, see Ijjas & Steinhardt (2019); Ijjas et al. (2017).

amount to no more than a consistency check.<sup>27</sup> On my account of testing, moreover, inflation does not fair well either, for it has, at least so far, not led to story-mediated measurements that refine the story. All that inflation adds, besides explaining away the fine tuning problem, is a nice story concerning the origin of the density fluctuations that seeded the acoustic oscillations and ultimately large scale structure. In this regard, inflation is commonly said to predict the power spectrum. Unfortunately, this prediction too is only a consistency check. The early universe had to have density perturbations, and inflation tells a nice origin story for these in terms of quantum fluctuations in the inflaton field that were then blown up to cosmological scales by the expansion, but until inflation can be made to do epistemic work in the form of licensing additional measurements, it is epistemically idle. Epistemic idleness is worrying, since it means that inflation has not been made to engage with other aspects of the cosmological story to lead to new evidence. Inflation is simply not, so far, probatively valuable.

A second worry is that the status of the fine tuning problem as a problem is not entirely clear. The issue here concerns the assessment that the initial condition (that inflation then explains away) had a very low probability. But initial conditions and dynamics are not independent, and so assessments of the probability of an initial condition requires knowledge of the relevant dynamics. But this is precisely what cosmologists are hoping to probe, for the physics of the Planck regime are unknown.<sup>28</sup> It seems, therefore, that it is premature to conclude that the primordial state was improbable or otherwise puzzling.

The third worry is, as far as I know, a new contribution to the philosophical literature. Recall that, in §3.3.2, I briefly discussed the importance of contingency for narrative histories in connection with Beatty's and Desjardins' work. Their insights concerned the fact that contingency is both very common in history and what make the narrative form indispensable. Contingency is often thought to make historical investigations especially challenging. This was Hempel's worry that I discussed in Ch.1, that historical trajectories are not law-like as they

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<sup>27</sup>See also Smeenk (2014); Earman & Mosterin (1999) for similar concerns. For philosophical arguments to the effect that inflation's explanatory power is evidentially sufficient for confirmational boost, see McCoy (2015, 2018); Friederich (2017, 2019).

<sup>28</sup>For more on this issue, see Smeenk (2014).

involve happenstance. But, as I argued, contingency in the sense that later states sensitively depend on earlier states, is an epistemic resource. But not all histories are so contingent. When a system reaches equilibrium, for example, its history is erased in the sense that its prior states cannot be inferred. Nature does not always cooperate with historical inquiry, for history is often erased. The past, then, is knowable in proportion to the degree to which later states depend on earlier ones.

Sensitive dependence is especially epistemically invaluable when the dynamics involved in earlier states are independently unknowable. And it is this kind of contingency that story-mediated measurement takes advantage of in order to individuate empirical constraints on theories concerning new dynamical regimes. But it is precisely here that I see a problem. The fine tuning problem is not a problem at all. In fact, it is an epistemic bounty bestowed on cosmologists the likes of which perhaps no other historical inquiry could ever dream of. Later states of the universe sensitively depend on the initial conditions to a mind-boggling degree, and it is this fact, that could have been different (yet another sense of contingency), that has all along been crucial to cosmology's success. Nature did not have to cooperate in this way, as other historical scientists will readily attest.

But by postulating inflation, cosmologists pull the plug on themselves. Inflation solves the fine tuning problem by postulating a dynamics wipes away the initial condition, for the state of the universe after inflation does not at all depend on what came before. It is precisely this that cosmologists take to be inflation's great success, for the initial energy density could have been any value and distributed in any way whatever, and inflation dilutes that all away. There is nothing left of the initial condition, and so whatever is observed in the later universe does not depend on the initial condition. So inflation cuts off empirical access to the very dynamical regimes that motivate cosmological inquiry. The issue is that inflation is an attractor dynamical solution to the fine tuning problem. Moreover, it is an especially bad one with respect to sensitive dependence on earlier times, for it makes the state represented by the CMB a necessary outcome, a uniform, flat state with space-like separated correlations is the unique



attractor that all prior states reach.

Note that I am not here arguing that inflation cannot be right or betting that inflation did not occur. It is possible that it did occur, for inflation is consistent with what is already known. Moreover, information loss due to attractor dynamics occurs in nature all the time. Instead, what I am suggesting is if inflation did occur, this would be bad news for cosmology's attempt to probe the Planck regime. Given these stakes, the evidential bar ought to be set very high for inflation, far higher than elegance and explanatory power. By settling for these non-empirical virtues, cosmologists may well be pulling the plug on the poor man's particle accelerator, cutting off the possibility of gaining story-mediated access to the beginning. Inflation, I fear, erases a tremendous epistemic bounty of sensitive dependence, and is thus the end of the beginning.

# Bibliography

Adams, F. C. (2010). The birth environment of the solar system. *Annual Review of Astronomy and Astrophysics*, 48, 47–85.

Aghanim, N., Akrami, Y., Ashdown, M., Aumont, J., Baccigalupi, C., Ballardini, M., Banday, A., Barreiro, R., Bartolo, N., Basak, S., et al. (2020). Planck 2018 results-vi. cosmological parameters. *Astronomy & Astrophysics*, 641, A6.

Albert, D. Z. (2000). *Time and Chance*. Harvard University Press.

Allegre, C. J., Manhès, G., & Göpel, C. (1995). The age of the earth. *Geochimica et Cosmochimica Acta*, 59(8), 1445–1456.

Anderl, S. (2016). Astronomy and astrophysics in the philosophy of science. In P. Humphreys (Ed.) *The Oxford Handbook of Philosophy of Sciences*. Oxford University Press.

Anderl, S. ((2018)). Simplicity and simplification in astrophysical modeling. *Philosophy of Science*, 85.5, 819–831.

Anscombe, G. E. M. (2000). *Intention*. Harvard University Press.

Baker, V. R. (1999). Geosemiosis. *Geological Society of America Bulletin*, 111(5), 633–645.

Beatty, J. (1984). Chance and natural selection. *Philosophy of Science*, 51(2), 183–211.

Beatty, J. (2006). Replaying life’s tape. *Journal of Philosophy*, 103(7), 336–362.

- Beatty, J. (2017). Narrative possibility and narrative explanation. *Studies in History and Philosophy of Science Part A*, 62, 31–41.
- Beatty, J. (forthcoming). What are narratives good for? *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*.
- Beatty, J., & Carrera, I. (2011). When what had to happen was not bound to happen: History, chance, narrative, evolution. *Journal of the Philosophy of History*, 5(3), 471–495.
- Beatty, J., & Desjardins, E. C. (2009). Natural selection and history. *Biology and Philosophy*, 24(2), 231–246.
- Belnap, N., & Müller, T. (2010). Branching with uncertain semantics: Discussion note on saunders and wallace, branching and uncertainty. *The British journal for the philosophy of science*, 61(3), 681–696.
- Bokulich, A. (Forthcoming). Calibration, coherence, and consilience in radiometric measures of geologic time. *Philosophy of Science*.
- Bouvier, A., & Wadhwa, M. (2010). The age of the solar system redefined by the oldest Pb–Pb age of a meteoritic inclusion. *Nature Geoscience*, 3(9), 637–641.
- Bovens, L., Hartmann, S., et al. (2003). *Bayesian epistemology*. Oxford University Press on Demand.
- Brennecka, G. A., & Wadhwa, M. (2012). Uranium isotope compositions of the basaltic angrite meteorites and the chronological implications for the early solar system. *Proceedings of the National Academy of Sciences*, 109(24), 9299–9303.
- Bromham, L. (2016). Testing hypotheses in macroevolution. *Studies in History and Philosophy of Science Part A*, 55, 47–59.
- Cameron, A., & Canup, R. (1998). The giant impact occurred during earth accretion. *29th Lunar and Planet. Sci.*, 1062.

- Canup, R. M., & Asphaug, E. (2001). Origin of the moon in a giant impact near the end of the earth's formation. *Nature*, 412(6848), 708–712.
- Carlson, R. W., Garnero, E., Harrison, T. M., Li, J., Manga, M., McDonough, W. F., Mukhopadhyay, S., Romanowicz, B., Rubie, D., Williams, Q., et al. (2014). How did early earth become our modern world? *Annual Review of Earth and Planetary Sciences*, 42, 151–178.
- Chambers, J., & Wetherill, G. (1998). Making the terrestrial planets: N-body integrations of planetary embryos in three dimensions. *Icarus*, 136(2), 304–327.
- Chambers, J. E. (2004). Planetary accretion in the inner solar system. *Earth and Planetary Science Letters*, 223(3-4), 241–252.
- Chang, H. (2004). *Inventing temperature: Measurement and scientific progress*. Oxford University Press.
- Chapman, R., & Wylie, A. (2016). *Evidential reasoning in archaeology*. Bloomsbury Publishing.
- Ćirković, M. M., & Perović, S. (2018). Alternative explanations of the cosmic microwave background: A historical and an epistemological perspective. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 62, 1–18.
- Cleland, C. E. (2001). Historical science, experimental science, and the scientific method. *Geology*, 29(11), 987–990.
- Cleland, C. E. (2002). Methodological and Epistemic Differences between Historical Science and Experimental Science. *Philosophy of Science*, 69(3), 447–451.
- Cleland, C. E. (2011). Prediction and Explanation in Historical Natural Science. *The British Journal for the Philosophy of Science*, 62(3), 551–582.

- Clerk Maxwell, J. (1864). On faraday's lines of force. *Transactions of the Cambridge Philosophical Society*, 10, 27.
- Condie, K. C. (2015). *Earth as an evolving planetary system*. Academic Press.
- Ćuk, M., Hamilton, D. P., Lock, S. J., & Stewart, S. T. (2016). Tidal evolution of the moon from a high-obliquity, high-angular-momentum earth. *Nature*, 539(7629), 402–406.
- Ćuk, M., & Stewart, S. T. (2012). Making the moon from a fast-spinning earth: a giant impact followed by resonant despinning. *science*, 338(6110), 1047–1052.
- Currie, A. (2015). Marsupial lions and methodological omnivory: Function, success and reconstruction in paleobiology. *Biology and Philosophy*, 30(2), 187–209.
- Currie, A. (2016). Hot-blooded gluttons: Dependency, coherence, and method in the historical sciences. *The British Journal for the Philosophy of Science*, (p. axw005).
- Currie, A. (2018). *Rock, Bone, and Ruin: An Optimist's Guide to the Historical Sciences*. MIT Press.
- Currie, A. (2019a). *Scientific knowledge and the deep past: history matters*. Cambridge University Press.
- Currie, A. (2019b). Simplicity, one-shot hypotheses and paleobiological explanation. *History and Philosophy of the Life Sciences*, 41(1), 10.
- Currie, A., & Sterelny, K. (2017). In defence of story-telling. *Studies in History and Philosophy of Science Part A*, 62, 14–21.
- Currie, A. M. (2013). Narratives, mechanisms and progress in historical science. *Synthese*, 191(6), 1–21.
- Dalrymple, G. B. (1994). *The age of the Earth*. Stanford University Press.
- Danto, A. C. (1965). Analytical philosophy of history. *Philosophy of Science*, 34(4), 388–392.

- Darwin, C. (1842). *The Structure and Distribution of Coral Reefs. Being the First Part of the Geology of the Voyage of the Beagle*. Smith, Elder and Company.
- Darwin, G. H. (1879). The determination of the secular effects of tidal friction by a graphical method. *Proceedings of the Royal Society of London*, 29, 168–181.
- Dawid, R. (2013). *String Theory and the Scientific Method*. Cambridge University Press.
- DePalma, R. A., Smit, J., Burnham, D. A., Kuiper, K., Manning, P. L., Oleinik, A., Larson, P., Maurrasse, F. J., Vellekoop, J., Richards, M. A., et al. (2019a). A seismically induced on-shore surge deposit at the kpg boundary, north dakota. *Proceedings of the National Academy of Sciences*, 116(17), 8190–8199.
- DePalma, R. A., Smit, J., Burnham, D. A., Kuiper, K., Manning, P. L., Oleinik, A., Larson, P., Maurrasse, F. J., Vellekoop, J., Richards, M. A., et al. (2019b). A seismically induced on-shore surge deposit at the kpg boundary, north dakota. *Proceedings of the National Academy of Sciences*, 116(17), 8190–8199.
- Desjardins, E. (2011a). Historicity and experimental evolution. *Biology and Philosophy*, 26(3), 339–364.
- Desjardins, E. (2011b). Reflections on path dependence and irreversibility: Lessons from evolutionary biology. *Philosophy of Science*, 78(5), 724–738.
- Desjardins, E. (2015). Historicity and ecological restoration. *Biology and Philosophy*, 30(1), 77–98.
- (Director), A. K. (1950). *Rashomon*. Daiei Film.
- Duhem, P. M. M. (1991). *The aim and structure of physical theory*. Princeton University Press.
- Earman, J. (2006). The past hypothesis: Not even false. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 37(3), 399–430.

- Earman, J. (2007). Aspects of determinism in modern physics. *The philosophy of physics*, 2.
- Earman, J. (2008). Pruning some branches from branching spacetimes. *Philosophy and Foundations of Physics*, 4, 187–205.
- Earman, J., & Mosterin, J. (1999). A critical look at inflationary cosmology. *Philosophy of Science*, 66(1), 1–49.
- Eckhardt, W. (2006). Causal time asymmetry. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 37(3), 439–466.
- Elga, A. (2001). Statistical mechanics and the asymmetry of counterfactual dependence. *Philosophy of Science*, 68(S3), S313–S324.
- Ereshefsky, M. (2010). What’s wrong with the new biological essentialism. *Philosophy of Science*, 77(5), 674–685.
- Ereshefsky, M., & Turner, D. (2019). Historicity and explanation. *Studies in History and Philosophy of Science Part A*.
- Forber, P., & Griffith, E. (2011). Historical reconstruction: Gaining epistemic access to the deep past. *Philosophy and Theory in Biology*, 3(201306), 1–19.
- Fox, C. (Under-Review). Story-mediated measurement in isotope geochemistry: The case of the origin of the moon. *British Journal of Philosophy of Science*.
- Friederich, S. (2017). Fine-tuning as old evidence, double counting, and the multiverse. *International Studies in the Philosophy of Science*, 31(4), 363–377.
- Friederich, S. (2019). A new fine-tuning argument for the multiverse. *Foundations of Physics*, 49(9), 1011–1021.
- Frodeman, R. (1995). Geological reasoning: Geology as an interpretive and historical science. *Geological Society of America Bulletin*, 107(8), 960–968.

- Gauthier-Lafaye, F., Holliger, P., & Blanc, P.-L. (1996). Natural fission reactors in the franceville basin, gabon. *Geochimica et Cosmochimica Acta*, 60(23), 4831–4852.
- Gee, H. (2000). *In search of deep time: Beyond the fossil record to a new history of life*. Cornell University Press.
- Goenner, H. F. (2010). What kind of science is cosmology? *Annalen der Physik*, 522(6), 389–418.
- Gould, S. J. (1986). Evolution and the triumph of homology, or why history matters. *American Scientist*, 74, 60–69.
- Gould, S. J. (2002). *The structure of evolutionary theory*. Harvard University Press.
- Grignon, C. (2012). Is cosmology a historical science? In *AIP Conference Proceedings 11*, vol. 1446, (pp. 333–337). American Institute of Physics.
- Gulick, S. P., Bralower, T. J., Ormö, J., Hall, B., Grice, K., Schaefer, B., Lyons, S., Freeman, K. H., Morgan, J. V., Artemieva, N., et al. (2019). The first day of the cenozoic. *Proceedings of the National Academy of Sciences*, 116(39), 19342–19351.
- Guth, A. H. (1981). Inflationary universe: A possible solution to the horizon and flatness problems. *Physical Review D*, 23(2), 347.
- Guth, A. H., & Steinhardt, P. J. (1984). The inflationary universe. *Scientific American*, 250(5), 116–129.
- Halliday, A. (2014). The origin and earliest history of the earth. *Planets, Asteriods, Comets and The Solar System*, 2, 149–211.
- Hanson, N. R. (1958). *Patterns of discovery: An inquiry into the conceptual foundations of science*, vol. 251. CUP Archive.



- Harper, W. L. (2011). *Isaac Newton's scientific method: turning data into evidence about gravity and cosmology*. Oxford University Press.
- Hempel, C. G. (1942). The Function of General Laws in History. *The Journal of Philosophy*, 39(2), 35–48.
- Hempel, C. G. (1988). Provisoes: A problem concerning the inferential function of scientific theories. *Erkenntnis*, 28(2), 147–164.
- Hofmann, A. W. (2008). Chemical geodynamics: The enduring lead paradox. *Nature Geoscience*, 1(12), 812.
- Horner, J. R., & Makela, R. (1979). Nest of juveniles provides evidence of family structure among dinosaurs. *Nature*, 282(5736), 296–298.
- Hossenfelder, S. (2018). *Lost in math: How beauty leads physics astray*. Basic Books.
- Hu, W. (????). Cmb power spectrum. <http://background.uchicago.edu/~whu/intermediate/map5.html>. Accessed: 2020-12-10.
- Hull, D. L. (1975). Central Subjects And Historical Narratives. *History and Theory*, 14(3), 253–274.
- Ijjas, A., & Steinhardt, P. J. (2019). A new kind of cyclic universe. *Physics Letters B*, 795, 666–672.
- Ijjas, A., Steinhardt, P. J., & Loeb, A. (2017). Cosmic inflation theory faces challenges. *Scientific American*, 316(2), 32–9.
- Inkpen, R. (2008). The philosophy of geology. *A companion to the philosophy of history and historiography*, (pp. 318–329).

- Isella, A., Benisty, M., Teague, R., Bae, J., Keppler, M., Facchini, S., & Pérez, L. (2019). Detection of continuum submillimeter emission associated with candidate protoplanets. *The Astrophysical Journal Letters*, 879(2), L25.
- Ismael, J. (2013). Decision and the open future. In *The future of the philosophy of time*, (pp. 159–178). Routledge.
- Jacobsen, B., Yin, Q.-z., Moynier, F., Amelin, Y., Krot, A. N., Nagashima, K., Hutcheon, I. D., & Palme, H. (2008). 26al–26mg and 207pb–206pb systematics of allende cais: canonical solar initial 26al/27al ratio reinstated. *Earth and Planetary Science Letters*, 272(1-2), 353–364.
- Janssen, M. (2002). Coi stories: Explanation and evidence in the history of science. *Perspectives on Science*, 10(4), 457–522.
- Jeffares, B. (2008). Testing times: Regularities in the historical sciences. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*, 39(4), 469–475.
- Jeffares, B. (2010). Guessing the future of the past. *Biology and Philosophy*, 25(1), 125–142.
- Kao, M. (2015). Unification and the quantum hypothesis in 1900–1913. *Philosophy of Science*, 82(5), 1200–1210.
- Kao, M. (2019). Unification beyond justification: a strategy for theory development. *Synthese*, 196(8), 3263–3278.
- Keller, G., Sahni, A., & Bajpai, S. (2009). Deccan volcanism, the kt mass extinction and dinosaurs. *Journal of biosciences*, 34(5), 709–728.
- Kenworthy, W., Scolnic, D., & Riess, A. (2019). The local perspective on the hubble tension: local structure does not impact measurement of the hubble constant. *The Astrophysical Journal*, 875(2), 145.

- Kitcher, P. (1989). Explanatory unification and the causal structure of the world. In P. Kitcher, & W. Salmon (Eds.) *Scientific Explanation*, (pp. 410–505). Minneapolis: University of Minnesota Press.
- Kleine, T., Mezger, K., Palme, H., & Münker, C. (2004). The W isotope evolution of the bulk silicate earth: constraints on the timing and mechanisms of core formation and accretion. *Earth and Planetary Science Letters*, 228(1-2), 109–123.
- Kleine, T., & Walker, R. J. (2017). Tungsten isotopes in planets. *Annual review of earth and planetary sciences*, 45, 389–417.
- Knox, L., & Millea, M. (2020). Hubble constant hunters guide. *Physical Review D*, 101(4), 043533.
- Kosso, P. (2001). *Knowing the past: Philosophical issues of history and archaeology*. Humanity Books.
- Kosso, P. (2009). Philosophy of Historiography. In A. Tucker (Ed.) *A Companion to the Philosophy of History and Historiography*, (pp. 7–25). Wiley-Blackwell.
- Kuukkanen, J.-M. (2015a). *Postnarrativist philosophy of historiography*. Springer.
- Kuukkanen, J.-M. (2015b). Why we need to move from truth-functionality to performativity in historiography. *History and Theory*, 54(2), 226–243.
- Lacki, B. C. (2015). Seti at planck energy: when particle physicists become cosmic engineers. *arXiv preprint arXiv:1503.01509*.
- Lewis, D. (1979). Counterfactual dependence and time's arrow. *Noûs*, 13(4), 455–476.
- Lock, S., Stewart, S., Petaev, M., Leinhardt, Z., Mace, M., Jacobsen, S., & Čuk, M. (2016). A new model for lunar origin: Equilibration with earth beyond the hot spin stability limit. In *Lunar and Planetary Science Conference*, 1903, (p. 2881).

- Lock, S. J., & Stewart, S. T. (2017). The structure of terrestrial bodies: Impact heating, corotation limits, and synestias. *Journal of Geophysical Research: Planets*, 122(5), 950–982.
- Lock, S. J., Stewart, S. T., Petaev, M. I., Leinhardt, Z., Mace, M. T., Jacobsen, S. B., & Cuk, M. (2018). The origin of the moon within a terrestrial synestia. *Journal of Geophysical Research: Planets*, 123(4), 910–951.
- Manchak, J. B. (2009). Can we know the global structure of spacetime? *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 40(1), 53–56.
- McCoy, C. (2018). The implementation, interpretation, and justification of likelihoods in cosmology. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 62, 19–35.
- McCoy, C. D. (2015). Does inflation solve the hot big bang models fine-tuning problems? *Studies in History and Philosophy of Modern Physics*, 51, 23–36.
- Melosh, H. (2014). New approaches to the moon’s isotopic crisis. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences*, 372(2024), 20130168.
- Mink, L. O., Fay, B., Golob, E. O., & Vann, R. T. (1987). *Historical Understanding*. Cornell University Press.
- Morbidelli, A., Lunine, J. I., O’Brien, D. P., Raymond, S. N., & Walsh, K. J. (2012). Building terrestrial planets. *Annual Review of Earth and Planetary Sciences*, 40.
- Morgan, J., Artemieva, N., & Goldin, T. (2013). Revisiting wildfires at the k-pg boundary. *Journal of Geophysical Research: Biogeosciences*, 118(4), 1508–1520.
- Morgan, J. V., Gulick, S. P., Bralower, T., Chenot, E., Christeson, G., Claeyes, P., Cockell, C.,

- Collins, G. S., Coolen, M. J., Ferrière, L., et al. (2016). The formation of peak rings in large impact craters. *Science*, 354(6314), 878–882.
- Morgan, M. S. (2017). Narrative ordering and explanation. *Studies in History and Philosophy of Science Part A*, 62, 86–97.
- Myrvold, W. C. (2003). A bayesian account of the virtue of unification. *Philosophy of Science*, 70(2), 399–423.
- Myrvold, W. C. (2017). On the evidential import of unification. *Philosophy of Science*, 84(1), 92–114.
- Myrvold, W. C. (2020). it would be possible to do a lengthy dialectical number on this. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*.
- Olsson, E. (2017). Coherentist Theories of Epistemic Justification. In E. N. Zalta (Ed.) *The Stanford Encyclopedia of Philosophy*. Metaphysics Research Lab, Stanford University, spring 2017 ed.
- Olsson, E. J. (2005). *Against coherence: Truth, probability, and justification*. Oxford University Press.
- O'Malley, M. A. (2016). Histories of molecules: Reconciling the past. *Studies in History and Philosophy of Science Part A*, 55, 69–83.
- Pahlevan, K., & Stevenson, D. J. (2007). Equilibration in the aftermath of the lunar-forming giant impact. *Earth and Planetary Science Letters*, 262(3-4), 438–449.
- Patterson, C. (1956). Age of meteorites and the earth. *Geochimica et Cosmochimica Acta*, 10(4), 230–237.

- Pearce, J. (2017). The unfolding of the historical style in modern cosmology: Emergence, evolution, entrenchment. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 57, 17–34.
- Pettinari, C., Marchetti, F., & Martini, D. (2003). Metal complexes as hydrogenation catalysts. In J. A. McCleverty, & T. J. Meyer (Eds.) *Comprehensive Coordination Chemistry II*, (pp. 75–139). Oxford: Pergamon.
- URL <https://www.sciencedirect.com/science/article/pii/B0080437486091258>
- Placek, T., & Belnap, N. (2012). Indeterminism is a modal notion: branching spacetimes and earman's pruning. *Synthese*, 187(2), 441–469.
- Pope, K. O., Baines, K. H., Ocampo, A. C., & Ivanov, B. A. (1997). Energy, volatile production, and climatic effects of the chicxulub cretaceous/tertiary impact. *Journal of Geophysical Research: Planets*, 102(E9), 21645–21664.
- Reichenbach, H. (1956). *The direction of time*, vol. 65. Univ of California Press.
- Rosales, A. (2017). Theories that narrate the world: Ronald a. fisher's mass selection and sewall wright's shifting balance. *Studies in History and Philosophy of Science Part A*, 62, 22–30.
- Roth, P. A. (1988). Narrative explanations: The case of history. *History and Theory*, 27(1), 1–13.
- Roth, P. A. (2012). The pasts. *History and Theory*, 51(3), 313–339.
- Roth, P. A. (2017a). Essentially narrative explanations. *Studies in History and Philosophy of Science Part A*, 62, 42–50.
- Roth, P. A. (2017b). Philosophy of History. In L. McIntyre, & A. Rosenberg (Eds.) *The Routledge Companion to Philosophy of Social Science*. Routledge.

- Ryden, B. (2017). *Introduction to cosmology*. Cambridge University Press.
- Salmon, W. C. (1984). *Scientific explanation and the causal structure of the world*. Princeton University Press.
- Schoene, B., Samperton, K. M., Eddy, M. P., Keller, G., Adatte, T., Bowring, S. A., Khadri, S. F., & Gertsch, B. (2015). U-pb geochronology of the deccan traps and relation to the end-cretaceous mass extinction. *Science*, 347(6218), 182–184.
- Schulte, P., Alegret, L., Arenillas, I., Arz, J. A., Barton, P. J., Bown, P. R., Bralower, T. J., Christeson, G. L., Claeys, P., Cockell, C. S., et al. (2010). The chicxulub asteroid impact and mass extinction at the cretaceous-paleogene boundary. *Science*, 327(5970), 1214–1218.
- Schupbach, J. N. (2015). The possibility of coherentism and the stringency of ceteris paribus conditions. *THEORIA. Revista de Teoría, Historia y Fundamentos de la Ciencia*, 30(1), 43–52.
- Schweitzer, M. H., Wittmeyer, J. L., & Horner, J. R. (2005). Gender-specific reproductive tissue in ratites and tyrannosaurus rex. *Science*, 308(5727), 1456–1460.
- Smeenk, C. (2014). Predictability crisis in early universe cosmology. *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, 46(1), 122–133.
- Smith, G. E. (2001). Jj thomson and the electron, 1897–1899. *Histories of the electron. The birth of microphysics*, (pp. 21–76).
- Smith, G. E. (2010). Revisiting accepted science: The indispensability of the history of science. *The Monist*, 93(4), 545–579.
- Smith, G. E. (2014). Closing the loop. In Z. Biener, & E. Schliesser (Eds.) *Newton and empiricism*, (pp. 31–70). Oxford University Press, USA.

- Smith, G. E., & Seth, R. ("in press"). *Brownian Motion and Molecular Reality: A Study in Theory-Mediated Measurement*. Oxford University Press.
- Sober, E. (1988). The principle of the common cause. In *Probability and causality*, (pp. 211–228). Springer.
- Sober, E. (1991). *Reconstructing the past: parsimony, evolution, and inference*. MIT press.
- Sober, E. (2001). Venetian sea levels, british bread prices, and the principle of the common cause. *The British Journal for the Philosophy of Science*, 52(2), 331–346.
- Sober, E. (In Press). Histories, dynamical laws, and initial conditions invariance under time-reversibility and its failure in markov processes, with application to the second law of thermodynamics and the past hypothesis. *Studies in the History and Philosophy of Modern Physics*.
- Starobinsky, A. A. (1982). Dynamics of phase transition in the new inflationary universe scenario and generation of perturbations. *Physics Letters B*, 117(3-4), 175–178.
- Sterelny, K. (2016). Contingency and history. *Philosophy of Science*, 83(4), 521–539.
- Stevenson, D. (1987). Origin of the moon-the collision hypothesis. *Annual review of earth and planetary sciences*, 15(1), 271–315.
- Thomson, J. J. (1977). *Acts and Other Events*. Cornell University Press.
- Toulmin, S. (1962). Historical inference in science: geology as a model for cosmology. *The Monist*, (pp. 142–158).
- Touma, J., & Wisdom, J. (1994). Evolution of the earth-moon system. *The Astronomical Journal*, 108, 1943–1961.
- Touma, J., & Wisdom, J. (1998). Resonances in the early evolution of the earth-moon system. *The Astronomical Journal*, 115(4), 1653.



- Trinquier, A., Birck, J.-L., Allègre, C., Göpel, C., & Ulfbeck, D. (2008). 53mn–53cr systematics of the early solar system revisited. *Geochimica et Cosmochimica Acta*, 72(20), 5146–5163.
- Tucker, A. (2004). *Our knowledge of the past: A philosophy of historiography*. Cambridge University Press.
- Turner, D. (2005). Local underdetermination in historical science. *Philosophy of Science*, 72(1), 209–230.
- Turner, D. (2007). *Making prehistory: Historical science and the scientific realism debate*. Cambridge University Press.
- Turner, D. D. (2019). *Paleoaesthetics and the Practice of Paleontology*. Cambridge University Press.
- Uebel, T. (2017). Philosophy of History and History of Philosophy of Science. *HOPoS: The Journal of the International Society for the History of Philosophy of Science*, 7(1), 1–30.
- Unger, R. M., & Smolin, L. (2014). *The singular universe and the reality of time*. Cambridge University Press.
- Unger, R. M., & Smolin, L. (2015). *The singular universe and the reality of time*. Cambridge University Press.
- Valley, J. W., Peck, W. H., King, E. M., & Wilde, S. A. (2002). A cool early earth. *Geology*, 30(4), 351–354.
- Wallace, D. (2010). Gravity, entropy, and cosmology: In search of clarity. *The British Journal for the Philosophy of Science*, 61(3), 513–540.
- Ward, W. R., Canup, R. M., & Rufu, R. (2020). Analytical model for the tidal evolution of the evection resonance and the timing of resonance escape. *Journal of Geophysical Research: Planets*, (p. e2019JE006266).

- White, H. (1996). The value of narrativity in the representation of reality. *Narratology: An Introduction*, (pp. 273–85).
- Whitehead, A. N. (2010). *Process and reality*. Simon and Schuster.
- Williams, J. P. (2010). The astrophysical environment of the solar birthplace. *Contemporary Physics*, 51(5), 381–396.
- Wise, M. N. (1979). The mutual embrace of electricity and magnetism. *Science*, 203(4387), 1310–1318.
- Wise, M. N. (2017). On the narrative form of simulations. *Studies in History and Philosophy of Science Part A*, 62, 74–85.
- Woodward, H. N., Fowler, E. A. F., Farlow, J. O., & Horner, J. R. (2015). Maiasaura, a model organism for extinct vertebrate population biology: a large sample statistical assessment of growth dynamics and survivorship. *Paleobiology*, 41(4), 503–527.
- Woodward, J. (1989). The causal/mechanical model of explanation. In P. Kitcher, & W. Salmon (Eds.) *Scientific Explanation*, (pp. 410–505). Minneapolis: University of Minnesota Press.
- Zahnle, K., Arndt, N., Cockell, C., Halliday, A., Nisbet, E., Selsis, F., & Sleep, N. H. (2007). Emergence of a habitable planet. *Space Science Reviews*, 129(1-3), 35–78.

# Curriculum Vitae

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## EDUCATION

|  |      |
|--|------|
| PhD., University of Western Ontario, Philosophy  | 2021 |
| Dissertation: “An Empiricist Defense of Scientific Stories About the Past” Committee:<br>Chris Smeenk (Supervisor), Eric Desjardins, Robert DiSalle, John Beatty, Gordon “Oz”<br>Ozinski (Examiners) |      |
| M.A., Tufts University, Philosophy   | 2012 |
| B.S., University of California, Los Angeles, Chemistry and Biochemistry  | 1997 |

**AREA OF SPECIALIZATION****AREA OF COMPETENCE**

History &amp; Philosophy of Science

Philosophy of Physics

Philosophy of Geosciences

Philosophy of Biology

Metaphysics &amp; Epistemology

Language &amp; Mind

Early Modern Philosophy

Logic

**PUBLICATIONS****Peer Reviewed Articles**

**Craig W. Fox**, “The Newtonian Equivalence Principle: How the relativity of acceleration led Newton to the equivalence of inertial and gravitational mass.” *Philosophy of Science* 83, no. 5 (2016): 1027-1038.

**Fox, C.**, Gueguen, M., Koberinski, A., and Smeenk, C., “Philosophy of Cosmology,” in *Oxford Bibliographies in Philosophy*, Ed. Duncan Pritchard. New York: Oxford University Press, 2019.

**Manuscripts Under Review (available upon request)**

“How the Earth got its Moon: On justly scientific stories.” (Under review with BJPS)

“In Search of Coherence.” (Under review with Boston Studies in the Philosophy and History of Science for a special volume)

**Work In Progress (available upon request)**

“Narratives in sciences that investigate the past: Or, how to build a timecapsule.”

“Rethinking the distinction between historical and experimental science.”

“Some Philosophical Prehistory of the Newtonian Equivalence Principle.”

“Back to the Beginning: Testing the theory of planetary accretion.”

“The End of the Beginning: Cosmic inflation undermines the project of cosmology.”

“The Emergence of a Habitable Earth: From Isotopic Abundances to the History of the Earth.”

## **AWARDS**

2012 Rotman Doctoral Fellowship

2010 Tufts Entrance Scholarship

2006 Los Angeles County’s Most Inspirational Teacher

## **INVITED TALKS**

2013 “In Pursuit of Foundations: Tracing Newton’s Conceptual Development,”  
Symposium on Newtonian Relativity, Stanford University, (June 2013).

## **CONFERENCE ACTIVITY**

### **Symposia Organized**

2014 “Newtonian Relativity,” Participants: Howard Stein, George Smith, Robert DiSalle, and Craig Fox, Philosophy Of Science Association (PSA), Biennial Meeting, Chicago, IL (Nov. 2014).

### **Upcoming Talks**

2021 “Looking into the past...,” Edelstein Center for Philosophy of Science.

2021 “The End of the Beginning: On a methodological crisis in cosmology,”  
PSA 2020.

### **Papers Presented**

- 2019 “Why Overdetermination of the past by the present doesn’t work in Isotope Geochemistry,” APA Central Division, Annual Meeting, Denver, CO (Mar. 2019).
- 2018 “On the Role of Stories in Isotope Geochemistry,” Symposium Title: “Exploring Model-Data Symbiosis in the Geosciences,” Participants: Alisa Bokulich, Craig Fox, Teru Miyake, and Wendy Parker, Philosophy Of Science Association (PSA), Biennial Meeting, Seattle, WA (Nov. 2018).
- 2018 “What is a trace?” Symposium Title: “Symposium: Author-Meets-Critics: Adrian Currie, ”Rock, Bone, and Ruin: An Optimist’s Guide to the Historical Sciences” (MIT Press, 2018),” Participants: Adrian Currie, Leonard Finkelmann, Craig Fox, Derek Turner, and Alison Wylie, Society for Philosophy of Science in Practice, University of Ghent, Ghent, Belgium (June 2018).
- 2017 “Making a timecapsule,” Philosophy Meets Paleobiology in the Badlands, Dinosaur Provincial Park, Alberta, Canada (August 2017).
- 2016 “The Newtonian Equivalence Principle: How the Relativity of Acceleration Led Newton to the Equivalence of Inertial and Gravitational Mass,” Gravity: Its History and Philosophy – 32nd Boulder Conference on the History and Philosophy of Science, University of Colorado, Boulder (October 2016).
- 2016 “Newtonian Relativity: How Newton Differentiated Inertial From Accelerative Mass,” Society For The History Of Philosophy Of Science (HOPOS) Biennial Meeting, University of Minnesota, Minneapolis (June 2016).
- 2014 “Inventing Mass: Newton’s process of conceptual revision culminating in his inventing mass,” Philosophy Of Science Association (PSA), Biennial Meeting, Chicago, IL (Nov. 2014).

### **Departmental Talks**

- 2014 “Fragility, a New Form of Coherence, and Narrative-Mediated Access: Philosophical insights gleaned from forensic science,” Graduate Student Colloquium Series at University of Western Ontario (Jan. 2014).
- 2013 “Soberian Mechanics: a theory of evolution qua Newtonian(-like) Theory of Forces” Graduate Student Colloquium Series at University of Western Ontario (Nov. 2013).

### **TEACHING EXPERIENCE**

#### **Teaching Assistant (University of Western Ontario)**

Philosophy & Climate Change (Spring, 2019)

Questions of the Day (Spring, 2018)

Business Ethics (Spring, 2016)

Big Ideas (Fall, 2015)

Questions of the Day (Spring, 2015)

Business Ethics (Fall, 2014)

Introduction to Philosophy (Fall, 2013 – Spring, 2014)

Introduction to Philosophy (Fall, 2012 – Spring, 2013)

#### **Teaching Assistant (Tufts University)**

Logic (Fall, 2011 – Spring, 2012)

Logic (Fall, 2010 – Spring, 2011)

**High School Science Teacher (Wilson High School, Long Beach, CA)**

Advanced Placement Biology Instructor (2001–2006)

Advanced Placement Chemistry Instructor (2006–2009)

**SERVICE TO PROFESSION****Journal Referee**

*Philosophy of Science* (2018)

*Philosophy of Science* (2017)

*Philosophy of Science* (2016)

*Science and Education* (2013)

**PUBLIC OUTREACH**

K–12 Education Outreach Director, Rotman Institute of Philosophy.

Coordinated public outreach for the Rotman Institute, focused on K–12 activities. Developed content for outreach programs, led outreach activities with high school and middle school students.

Designed exhibits and gave lectures for a public exhibition celebrating the 100<sup>th</sup> anniversary of Einstein's General Relativity.